THREE ESSAYS ON STATE AND LOCAL PUBLIC FINANCE

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

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August 2018

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Cornell University 2018

ABSTRACT

This dissertation is comprised of three essays, separated into three chapters.

In the first chapter, I estimate the effect of education spending on district-level student outcomes in 24 states by leveraging changes in revenue driven by property value variation. I interact state-level changes in property values with fixed school finance formulas that measure how state aid and local revenue respond to those changes to create a simulated instrument for school spending. I measure a leave-one-out mean change in property values for school districts with administrative data I compiled from individual states on property values for over 7,000 school districts. The instrument is highly predictive of changes in revenue and spending. My estimates suggest that a 10 percent increase in spending improves graduation rates by 2.1 to 4.4 percentage points and student test scores by 0.05 to 0.09 standard deviations. These results suggest that market variation in property values affects student outcomes through existing school finance formulas.

The second chapter documents the existence and magnitude of an intergenerational effect of cigarette taxes on smoking initiation, which flows from older to younger members of the community. We estimate a discrete-time hazard model with cigarette taxes from before an individual is at risk of beginning smoking (from birth to age seven). A \$0.25 increase in cigarette taxes during childhood decreases the hazard of later initiating smoking by 12.5 percent. We

find no evidence that contemporaneous cigarette taxes affect smoking initiation. Prior work understates the effect of cigarette taxes on smoking by not considering this intergenerational channel.

The final chapter explores whether increased resources for traditional public school districts helps stem the flow of students to charter schools in Ohio. To do this, we separately examine the impact of school resource expansions for capital projects and those for general purposes (current operating expenses, instructional expenditures, support services, etc.) on the fraction of potential traditional public school students attending a charter school, student outcomes, and housing values. Both types of resource expansion reduce the fraction of potential students attending digital charters but have no effect on students lost to brick-and-mortar charters or student achievement.

BIOGRAPHICAL SKETCH

Corbin earned a B.A. in Economics and a B.S. in Mathematics from Brigham Young University in 2012 and a M.A. in Economics from Cornell University in 2016. Corbin is an applied microeconomist whose research explores the role of public finance in important education, health, and labor market outcomes. The topics of his current work include state and local education finance, property taxes, and cigarette taxes.

ACKNOWLEDGEMENTS

I am grateful for the guidance and instruction of my dissertation committee: Michael Lovenheim, Donald Kenkel, and Maria Fitzpatrick. The combination of Mike's high expectations and infectious optimism have inspired my research and professional development. His attention to detail and generous time spent reading and obliterating my drafts with comments have made me a better researcher and writer. I benefited greatly from Don's advise and experience. He always helped me feel comfortable and know what to expect in a number of academic and professional situations. Researching for Maria exposed me to new research ideas and resources and taught me the importance of good communication. I am also grateful to the faculty in the Department of Economics and the Department of Policy Analysis & Management for uncountable moments of teaching and advice, especially additional opportunities to do research as a research assistant to Matt Hall and Doug Miller.

This dissertation was supported, in part, by the C. Lowell Harriss Dissertation Fellowship from the Lincoln Institute of Land Policy. I would also like to thank Jason Cook, Rich Patterson, Todd Jones, Jeff Swigert, Barton Willage, and Alex Willén for their friendship and supportive conversations about research and surviving the job market.

Finally, I give deep thanks to my wife Julie for her support and patience through the years of long hours and late nights required to complete this dissertation. She was always there to compensate in the areas I lagged behind to provide our family with what they needed and always push us to be better. This dissertation is dedicated to Julie, Theo, Ruby, and Lucas, who were the motivation to accomplish this goal.

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CHAPTER 1

THE EFFECT OF EDUCATION SPENDING ON STUDENT ACHIEVEMENT: EVIDENCE FROM PROPERTY VALUES AND SCHOOL FINANCE RULES

1.1 Introduction

The United States spends roughly 3.7 percent of its GDP on public K-12 education.[1] Public schools were historically funded primarily through local revenue, the majority of which came from local property taxes, as shown in Figure 1.1. Reliance on property taxes led to large disparities in school spending across districts based on local income levels and property wealth. In the 1970s and 1980s, state courts started ruling that these disparities made local funding of school districts unconstitutional. States responded by enacting funding formulas that reduced the relationship between local property values and school resources to equalize spending across districts. In many cases, formulas explicitly take into account property values to provide more funding for low-property-wealth districts. Recent research leverages the policy variation from these school finance reforms and finds positive effects of spending on high school completion and other long run outcomes (Jackson, Johnson, and Persico, 2016), as well as student test scores (Lafortune, Rothstein, and Schanzenbach, 2018). Despite the increased importance of state funds, property values are still a major component of school district spending in most states. While much effort has been devoted

[1] In FY2014, GDP was $17.43 trillion (U.S. Bureau of Economic Analysis, 2017) and total spending on public K-12 was $634 billion (U.S. Department of Education National Center for Education Statistics, 2016b).

to estimating the effect of school spending and quality on property values (Oates, 1969; Black, 1999; Bayer, Ferreira, and McMillan, 2007; Ries and Somerville, 2010), little is known about how changes in property values affect student achievement by influencing school district revenues.

I address this gap by estimating the effect of education spending, driven by property value fluctuations, on district-level student outcomes in 24 states. Observational analyses of changes in school spending on student outcomes are biased by changes in spending driven by other factors that also determine student achievement, such as the demographic composition of students or parental support of education. To overcome such sources of endogeneity, I create a simulated instrument for school revenue by interacting state-level changes in property values with fixed school finance formulas that measure how state aid and local property tax revenue respond to changes in district property wealth. My estimates show that a 10 percent increase in simulated revenue increases spending by 1 to 2 percent, which suggests that administrators do not perfectly adjust tax rates and other school finance parameters to offset changes in revenue driven by changes in property values. Simulated revenue is a valid instrument for district spending as long as changes in unobserved factors that affect both student outcomes and spending are not systematically related to changes in property values differentially across districts with different base-year finance policies. I show that measures of student composition that are likely related to student achievement (e.g. fraction of students eligible for free lunch, fraction of minority students) trend similarly for districts with different combinations of high and low initial property tax rates and property wealth, which provides support for the validity of my instrument.

The student outcomes I examine are graduation rates and test scores. Graduation rates are based on district-level information from the National Center for Education Statistics Common Core of Data (CCD) from 1998 to 2010. I use nationally-comparable math and reading test scores for 4th and 8th graders, aggregated to the district level, from the Stanford Education Data Archive (SEDA). These test scores are available from 2009 to 2013.

Using my measure of simulated revenue as an instrument for spending in a two-stage least squares framework, I find that a 10 percent increase in spending in the final two years of high school increases graduation rates by 2.1 to 4.4 percentage points. These estimates are comparable to, but slightly larger than, those found in recent papers using variation from school finance reforms.[2] Two potential reasons my estimates are larger than those using variation from school finance reforms include the impact of spending in high income areas and the differential effect of spending cuts. My estimates suggest that the benefits of increased spending are concentrated in districts with high incomes. Because school finance reforms predominantly affect low-income districts, estimates for high-income districts using this variation likely do not have a strong first stage. My instrument is strong for both low- and high-income districts, so I am able to reliably estimate the effect of spending in high-income areas. Also, my estimates are identified off of both expansions and reductions in resources, while school finance reforms provide only expansions. This means my estimates are

[2] Jackson, Johnson, and Persico (2016) find that a 10 percent increase in spending from school finance reforms, across 12 years of school, increases graduation rates by 3.55 percentage points. Candelaria and Shores (2017) replicate this result and also find an effect size of 5 percentage points when restricting to the highest poverty districts. Each of these papers uses the number of graduates per 8th grader (four years prior) as a proxy for graduation rates, which is the measure for which I get the largest magnitude estimate of 5.1 percentage points.

less sensitive to bias from secular trends.

For test scores, I find that increased average spending 5 to 8 years prior to the tests increases 4th and 8th grade math and reading scores. A 10 percent increase in spending increases 4th grade math scores by about 0.08 standard deviations (SDs), 4th grade reading scores by 0.09 SDs, 8th grade math scores by 0.09 SDs, and 8th grade reading scores by .05 SDs. Importantly, spending from before students even enter school improves their future test scores, which suggests that investments in schooling inputs have a lasting effect. Whether or not increased spending improves student test scores is not a new question in the literature. Since Coleman et al. (1966) there have been dozens of studies attempting to estimate the education production function (Todd and Wolpin, 2003). This debate has been contentious and has not yet lead to a consensus (Hanushek, 2003; Krueger, 2003). This lack of consensus is driven by the endogeneity between spending and student outcomes and the difficulty in identifying a causal relationship. My estimates are consistent with the most recent, well-identified estimates, which suggest that increasing total school resources does indeed improve test scores (Lafortune, Rothstein, and Schanzenbach, 2018).

My primary contribution is new empirical evidence for the effect of spending on student achievement using more regular, high-frequency variation than in past studies. My estimates are identified by year-to-year variation in funding within the existing policy structure rather than large, targeted overhauls of those structures as in school finance reforms. The findings suggest that large, structural changes in school finance formulas are not prerequisites for spending to affect student outcomes.

As a result of my novel identification strategy, I make three additional contributions. First, I compile a new administrative data set from 24 states that contains property values for over 7,000 school districts. This information is necessary for my identification strategy and valuable for other research questions in local public finance. Second, I simulate expected changes in revenue based solely on changes in property values by coding up the key features of school finance formulas as of 1999. Finally, the reduced-form of my estimates provides the first evidence of a causal effect of market fluctuations in property values on student achievement.[3] My first-stage estimates show that increased property values significantly increase school revenue through both local sources and state aid. Although school finance reforms decreased the cross-sectional relationship between local property values and school spending, there remains a significant time-series relationship that influences student outcomes.

My findings have several implications for policy. First, I find that changes in property values indirectly affect student outcomes by changing the level of available resources. This connection means that volatile housing prices can lead to volatile student outcomes, which is an undesirable outcome for school districts. This suggests that policymakers can improve student outcomes by allowing school districts opportunities to smooth spending through borrowing and saving. Loosening the credit constraints of school districts can help insulate against volatile housing markets and the harmful effects of spending cuts. Second, a concern with increasing education spending is that we are already at the "flat of the curve," meaning, spending is sufficiently high that the marginal effect of each additional dollar is near zero. My results

[3] Davis and Ferreira (2017) estimate the effect of housing values on school finances, but are limited in the test score outcomes they are able to investigate due to the timing restriction of their identification strategy.

suggest that, even during the 2000s when spending per pupil was historically high, the United States has not yet reached that theoretical plateau. Finally, increased spending improves test scores even when the spending occurs prior to when students enter school. This relationship suggests that there are durable or delayed effects of investments in school inputs on test scores. This is another reason that simply comparing the current level of funding with contemporaneous outcomes is not likely to capture the relationship between spending and student achievement.

The next section provides background information about the prior literature, property taxes, and school finance programs in the United States. The data is discussed in Section 1.3. Section 1.4 explains my simulated instrument and empirical strategy. I present my results in Section 1.5, and Section 1.6 concludes.

1.2 Background

1.2.1 Prior Literature

A large literature attempts to apply the framework of production technologies to the education process. These studies estimate the relative importance of primary inputs, which include an individual endowment of ability and the influence of families, peers, and schools (Todd and Wolpin, 2003). The output of the education process is cognitive and noncognitive skills that culminate in persistence in education and eventual labor market earnings.

The first study to examine the relative importance of school inputs and family inputs on student achievement was Coleman et al. (1966). Coleman finds that family characteristics explain the majority of variation in test scores and spending explains little. At the time, people took these results to mean that schools did not matter and the variation in student outcomes is a result of family and peer effects. The methodology used in the analysis were severely criticized at the time (Bowles and Levin, 1968; Cain and Watts, 1970; Mosteller and Moynihan, 1972). Even so, the counterintuitive results in Coleman et al. (1966) ignited decades of hotly-debated research into the relationship between spending and student achievement, which find contrasting evidence (see Hanushek, 2003; Krueger, 2003). Most of these studies require strong assumptions to be interpreted as causal because they lack exogenous variation in spending.

Recently, more well-identified studies leverage experimental or quasi-experimental variation in school inputs to examine their effect on student outcomes. These inputs include class size (Krueger, 1999; Angrist and Lavy, 1999; Hoxby, 2000; Krueger and Whitmore, 2001; Chetty et al., 2011), teacher quality (Chetty et al., 2011), and capital spending (Cellini, Ferreira, and Rothstein, 2010; Martorell, Stange, and McFarlin, 2016; Hong and Zimmer, 2016). Others exploit large changes in spending due to school finance reforms (SFRs). SFRs increased spending and decreased spending gaps between high- and low-income school districts by 19 to 34 percent (Murray, Evans, and Schwab, 1998). Card and Payne (2002) find that increased spending from SFRs decreased the gap in SAT scores across family background groups. Jackson, Johnson, and Persico (2016) find that increased per-pupil spending increased educational attainment and adult earnings, and Hyman (2017) finds that Michigan's SFR improved college-going and completion.

Most relevant to the present study are those that use variation in spending from SFRs to examine the relationship between spending and student outcomes such as graduation rates, test scores, and labor market outcomes. Jackson, Johnson, and Persico (2016) use individual-level data from the Panel Study of Income Dynamics to link adults to the school district in which they grew up to measure the effect of court-ordered school finance reforms on long-run outcomes. They find that a 10% increase in spending increased high school graduation rates by 7.1 percentage points, years of schooling by 0.3 years, and adult earnings by 7 percent. Using the same district-level graduation data from the CCD as the current study, they find that a 10% increase in spending increased graduation rates by 3.55 percentage points. Candelaria and Shores (2017) replicate this finding and also estimate that a 10% increase in spending increased graduation rates by 5 percentage points in the quartile with the highest fraction of free-lunch eligible students. My estimates are in the same range as these and come from a completely different source of variation in school district spending.

Most SFR studies that examine test scores do so in individual states. These include Clark (2003) who finds no test score gains in Kentucky, and Papke (2005) who finds increased proficiency scores in Michigan. The one study to use SFRs to examine the effect of spending on standardized test scores nationwide is Lafortune, Rothstein, and Schanzenbach (2018). Using restricted-access individual-level information from the state NAEP, they create state-level measures of test score disparities between low and high income school districts. They find that after reforms, low-income districts close the gap between their test scores and those in high-income districts by 0.1

standard deviations, which gives an effect size of 0.12 to 0.24 standard deviations per $1,000 in annual spending per pupil. My paper is the first to estimate the causal effect of spending on test scores at the district level that are nationally comparable.

1.2.2 Property Taxes

Most school districts are governed by a school board with authority to levy property taxes for school funding. This taxing authority is limited by statute and the approval of local voters. Property taxes are *ad valorem* taxes determined by multiplying the aggregate taxable value of property in the district by the property tax rate. The tax rate is often reported in "mills," or thousandths of a dollar. That is, a property tax rate of 1 mill corresponds to a fraction of $\frac{1}{1000} = 0.001$. Nearly all property tax-imposing jurisdictions tax real estate such as residential and commercial properties. Other common types of taxable property include motor vehicles, agricultural land, mineral wealth, and certain types of property used in business such as machinery.

States impose a number of restrictions, known as tax and expenditure limits (TELs), on the property taxing behavior of local governments. These restrictions determine the taxable value of property and restrict the allowable level and growth of property tax revenue. TELs were mostly enacted in reaction to the property tax revolts of the 1970s and 1980s as a way of codifying the *de facto* tax breaks that homeowners were already receiving prior to the institution of rigorous assessment practices. States determine the fraction of property that is subject to taxation – called the assessment rate – for each type of property. Historical or religious buildings are exempt from taxation and their assessment rate is zero. Other properties are partially exempt and given a lower assessment rate such as homesteads and homes owned by low-income seniors or veterans. Other TELs restrict taxing behavior to reduce the tax burden and limit volatility in property tax payments. To reduce the tax burden, state impose fixed tax limits, such as maximum or minimum millage rate requirements. Some states also limit the annual change in assessed value, revenue, or tax payments.

Previous studies find that introducing TELs decreased school inputs and weakened student outcomes. Student-teacher ratios increased significantly as a result of Oregon's tax limitation (Figlio, 1998). Figlio and Rueben (2001) also find that TELs reduce the test scores of education majors, and presumably their effectiveness as teachers. Downes, Dye, and McGuire (1998) find that the introduction of TELs in Illinois led to a small reduction in 3rd grade math scores, but found no effect for reading scores. Rather than estimate the effect of moving to a new set of TELs, I include the dynamic limits in my simulated instrument since they are predetermined responses to large fluctuations in property values. District fixed effects account for the fixed limits. These rules represent important differences across states in how and when increased property values translate into revenue. As of 1999, 19 states had some sort of dynamic limit on the growth of property tax revenue.[4]

1.2.3 School Finance

School districts receive about 45 percent of their funding from local sources, 46 percent from state sources, and the remaining 9 percent from federal sources (U.S.

[4] Table A.1 lists each of the dynamic TELs as of 1999.

Department of Education National Center for Education Statistics, 2016a). Eighty percent of local revenue is generated through property taxes. The majority of state revenue for education comes from income and sales taxes.[5] State funds are distributed to local school districts based on a formula set by the state legislature, usually on a per-pupil basis. In addition to the student counts, state finance formulas depend on the ability of school districts to raise local revenue, usually measured by property wealth. Other supplementary funds are distributed based on program offerings through categorical grants or special circumstances like geographically large districts that need additional funding for transportation.

Funds are available to school districts based on the following relationship:

$$R_t^d = L_t(\tau_t^d \times W_t^d) + S_t(\tau_t^d, W_t^d, \mathbf{\Gamma}_t^d) + Fed_t(\mathbf{\Lambda}_t^d), \tag{1.1}$$

where R_t^d is the sum of revenue from all sources for district d and year t. Local revenue, $L_t(\cdot)$, is a function of the revenue generated by applying the school property tax rate to the property wealth within a district along with any tax and expenditure limits.[6] Thus, W_t^d is the market value of property, τ_t^d is the millage rate, and $L_t(\cdot)$ accounts for non-linearities imposed by TELs. The effective tax rate is the fraction of market value of property that is received as property tax revenue, which differs from the statutory property tax rate for several reasons. It is also useful to define ℓ_t^d such that $\tau_t^d \times \ell_t^d W_t^d = L_t^d(\tau_t^d \times W_t^d)$ for interior values (where the non-linearities are not binding). The state revenue function, $S_t(\cdot)$, depends on local tax effort –

[5] In some states property tax revenue for schools is treated more like a state revenue source because states either directly collect the property tax, or receive funds from local districts that they then redistribute.

[6] Although TELs are imposed by the state, they directly affect the collection of local revenue.

measured by τ_t^d – and tax capacity – measured by W_t^d – as well as characteristics of the district, $\mathbf{\Gamma}_t^d$, such as student counts and participation in certain educational programs like special education or free or reduced-price lunch. Transfers between state and local governments are captured in S_t, so if states redistribute revenue from high-wealth to low-wealth areas, then S_t can be negative. Federal revenue, $Fed_t(\cdot)$, is a function of district characteristics, $\mathbf{\Lambda}_t^d$, that may or may not also be included in $\mathbf{\Gamma}_t^d$, depending on the state.

As shown in Equation 1.1, there are several sources of potential variation in total revenue. For example, states can make legislative adjustments to the funding formula (changes to $S_t(\cdot)$) or districts can adjust the property tax rate (changes to τ_t^d). School finance reforms constitute a fundamental change in the form of S_t that is above and beyond small adjustments to the parameters of the existing system. Hoxby (2001) uses the term inverted tax price to denote "the dollars that a district gets to spend if it raises one dollar in local revenue" regardless of whether that dollar is generated by a change in the tax rate or the tax base. Here, I separate these two determinants of revenue and use the term tax price to refer specifically to $\frac{\partial R_t^d}{\partial \tau_t^d}$, or the change in revenue given an increase in the tax rate. I separately define the wealth price as $\frac{\partial R_t^d}{\partial W_t^d}$, or the marginal change in revenue given a unit increase in property wealth. The wealth price depends on how W_t^d interacts with each revenue source. By differentiating both sides of Equation 1.1, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \frac{\partial L_t^d}{\partial W_t^d} + \frac{\partial S_t^d}{\partial W_t^d}. \tag{1.2}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \frac{\partial L_t^d}{\partial \tau_t^d} + \frac{\partial S_t^d}{\partial \tau_t^d}. \tag{1.3}$$

Thus, the wealth price and the tax price reflect both changes in local property tax revenue and direct responses in state aid. Districts choose their tax rate based on the tax price, but are unlikely to consider the dynamic effect feeding back through the wealth price in the future.

Nearly every state uses a foundation program, district power equalization, or a combination of the two.[7] The most common school finance policy is the foundation program, which is used in over 40 states. The goal of a foundation plan is to provide adequate funding by guaranteeing an amount of funding per pupil. The guaranteed amount of spending per pupil is called the foundation level. To qualify for state aid, districts are responsible for contributing a local share defined by applying the foundation tax rate, τ^f, to their taxable property value. Foundation programs do not preclude districts from raising additional funds by taxing above the foundation tax rate. Generally, foundation programs provide state aid based on

$$S_t^d = \max\{0, \text{Foundation}_t \times ADM_t^d - L_t^d(\tau_t^f \times W_t^d)\}, \tag{1.4}$$

where the guaranteed amount of funding is the product of Foundation_t (the statewide foundation level; dollars per student), and ADM_t^d (average daily membership; the number of students in the district). If we let $F_t^d = \text{Foundation}_t \times ADM_t^d$, then the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - \tau_t^f) \times \ell_t^d, & W_t^d < \frac{F_t^d}{\ell_t^d \tau_t^d} \\ \tau_t^d \ell_t^d, & W_t^d > \frac{F_t^d}{\ell_t^d \tau_t^d} \end{cases} \tag{1.5}$$

[7] Table A.2 summarizes the type of school finance programs used in each state. Hawaii's single school district is entirely funded by the state and does not receive property tax revenue. North Carolina provides flat grants to districts, which can be supplemented by local property tax revenue.

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d, \qquad \tau_t^d \neq \tau_t^f. \tag{1.6}$$

The relationship between revenue and property wealth and between revenue and the tax rate are shown in panel A of Figure 1.2. The dashed line shows revenue with no state aid, so the distance between the dashed and solid line represents the amount of state aid. For a district with no property wealth, revenue is exactly the foundation guarantee, F_t^d. As wealth increases, revenue increases by $(\tau_t^d - \tau_t^f) \times \ell_t^d$, or how far the district's tax rate is above the foundation tax rate. This continues until wealth is above $\frac{F_t^d}{\ell_t^d \tau_t^d}$, at which point state aid is zero. Districts with a tax rate below the foundation tax rate receive no state aid and districts with a tax rate above receive aid in the amount $F_t^d - \tau_t^f \ell_t^d W_t^d$, or the guaranteed amount minus the amount of local revenue generated by taxing the foundation rate. Note that there is no upper limit to the local tax rate at which districts receive state aid.

The second most common set of school finance policies are district power equalization programs. To help subsidize funding for low-wealth districts, equalization programs guarantee an amount of revenue per mill regardless of district property wealth. Generally, equalization plans distribute funds based on

$$S_t^d = L_t^d \left(\tau_t^d \times \left(\max\{W_t^d, W_t^*\} - W_t^d\right)\right), \tag{1.7}$$

where W_t^* is the guaranteed wealth level. The state tops local revenue up to what a district with the guaranteed wealth level would get by levying the same tax rate.

The wealth price for this plan is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} 0, & W_t^d < W_t^* \\ \tau_t^d \ell_t^d, & W_t^d > W_t^*, \end{cases} \tag{1.8}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \max\{\ell_t^d W_t^*, \ell_t^d W_t^d\}. \tag{1.9}$$

Panel B of Figure 1.2 the relationship between revenue and wealth and between revenue and the tax rate for a general district power equalization plan. Districts with property wealth less than the guaranteed level receive $\tau_t^d \ell_t^d W_t^*$ in revenue. As property wealth increases, the amount of revenue does not change, but the fraction of revenue from state aid decreases until wealth reaches the guaranteed wealth level and state aid becomes zero. Revenue increases by $\max\{\ell_t^d W_t^*, \ell_t^d W_t^d\}$ as the tax rate increases.

Below, I provide examples from New Mexico and Georgia to explain how I calculate the wealth and tax price.[8] I use the term $wADM_t^d$ to refer to weighted average daily membership or the weighted number of students.[9] These examples also help show why the wealth price varies between districts within states as well as across states. To emphasize this point, Figure 1.3 shows the distribution of wealth price for each of the states in my analysis. The within state variation comes from differences in property tax rates and property wealth across districts. Variation across states also depends on these factors but is additionally driven by differences in the

[8]The school finance formulas for other states in my sample are described in detail in Appendix A.3.

[9]I use the same notation for districts across states, but in constructing the state finance formula I take into account the substantial differences in how states weight students in different grades or programs.

state's funding formulas. This variation is not obvious based on the finance formulas and may not even be apparent to districts themselves. Nevertheless, there is a large amount of both within- and between-state variation in the wealth price, which I exploit in my identification strategy.

Foundation Example: New Mexico

New Mexico has a simple foundation program established by the New Mexico Public School Finance Act of 1974. The foundation tax rate is 0.5 mills, so local revenue is $L_t^d = \tau_t^d \times \ell_t^d W_t^d$ and state revenue is

$$S_t^d = \text{Foundation}_t \times wADM_t^d - 0.0005 \times \ell_t^d W_t^d. \tag{1.10}$$

Although there is no limitation in the law that requires S_t^d to be positive, the finance rules and characteristics of districts are such that this is not negative in practice. Total revenue is then given by

$$R_t^d = \text{Foundation}_t \times wADM_t^d + (\tau_t^d - 0.0005) \times \ell_t^d W_t^d + Fed_t^d. \tag{1.11}$$

This gives a wealth price of

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.0005) \times \ell_t^d. \tag{1.12}$$

Thus, without any action by the school district, revenue increases by a set fraction of any additional property wealth and depends directly on the local tax rate and the foundation tax rate. Similarly, the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d \tag{1.13}$$

which depends on the level of property wealth.

Foundation + Equalization Example: Georgia

Georgia's Quality Basic Education Act provides funds per weighted pupil based on a foundation program with an optional equalization component. The foundation tax rate is 5 mills. The equalization component provides the difference between the revenue generated from 5 to 8.25 mills and what would have been generated by a district with that same millage rate and property wealth as a district at the 90th percentile of wealth in the state. Local revenue is $L_t^d = \tau_t^d \times \ell_t^d W_t^d$ and state revenue is

$$S_t^d = \text{Foundation}_t \times wADM_t^d - 0.005 \times \ell_t^d W_t^d \\ + \begin{cases} 0.00325 \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ (\tau_t^d - 0.005) \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ 0 & \text{if } \ell_t^d W_t^d \geq W_t^{90} \end{cases}, \tag{1.14}$$

where W_t^{90} is the 90th percentile of wealth across districts in the state. There is no statutory limitation on S_t^d that keeps this value from being negative, but the total state aid given to all districts is restricted by limiting the local share to less than 25 percent of the total foundation guarantee aggregated across all districts. Total revenue is then given by

$$R_t^d = \text{Foundation}_t \times wADM_t^d + (\tau_t^d - 0.005) \times \ell_t^d W_t^d$$
$$+ \begin{cases} 0.00325 \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ (\tau_t^d - 0.005) \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ 0 & \text{if } \ell_t^d W_t^d \geq W_t^{90}. \end{cases} \tag{1.15}$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - 0.00825) \times \ell_t^d & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ 0 & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ \left(\tau_t^d - 0.005\right) \times \ell_t^d & \text{if } \ell_t^d W_t^d \geq W_t^{90} \end{cases} \tag{1.16}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} \ell_t^d W_t^d & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ \ell_t^d W_t^d + \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ \ell_t^d W_t^d & \text{if } \ell_t^d W_t^d \geq W_t^{90}. \end{cases} \tag{1.17}$$

For districts with property wealth below the 90th percentile, state aid decreases and local revenue increases from each additional dollar of property wealth. If they levy between 5 and 8.25 mills the change in state and local funds cancel each other out and total revenue does not change. Once they levy above 8.25 mills the increase in local funds is enough to compensate for the decrease in state aid and total revenue increases. Districts at or above the 90th percentile of wealth are not affected by the equalization component and only experience the foundation portion of the plan, so they get a fraction of each additional dollar of property wealth as revenue.

1.3 Data

1.3.1 Data Sources

This project draws data from several sources. My primary source of data is the National Center for Education Statistics' Common Core of Data (CCD). I supplement the CCD with additional district-level information including a database of district property values collected from individual states, school finance formulas coded from legal records, test scores, and median household income.

NCES Common Core of Data

The CCD is a comprehensive, national database of all public schools and school districts in the United States. Fiscal information is available annually back to 1995 and non-fiscal characteristics are available back to 1987. The variables I use from the CCD include expenditures, revenues, and the number of students in several race categories and in certain educational programs. Expenditures are reported in a number of categories including instructional spending, capital outlays, and administrative spending.[10] Revenues are reported in several fine categories and aggregated to local, state, and federal sources. One subcategory I use to construct my instrument

[10] I consider both log spending and spending per pupil in my analysis. Results are consistent between the two measures, but I primarily discuss the log spending measures.

is property tax revenue, which I divide by district property wealth to calculate the effective tax rate. The endogenous variable of interest that I instrument for in my identification strategy is total expenditures. I use student count data to create controls for total student enrollment, the fraction of students who are black or Hispanic, have an individualized education plan (IEP)/are in special education, or are eligible for free or reduced price lunch.

School District Property Wealth Database

The CCD does not include a measure of district property wealth, which is crucial for my estimation strategy. Most states have an agency (usually a Department of Revenue or Department of Taxation) that oversees local auditors who assess values for property tax purposes. Due to this responsibility, summaries of property values at each geographic level of taxation (e.g. county, municipality, school district) are often available from these state agencies. I collected this information individually from states and created the first school district-level database of property values covering years 1999-2014.[11] This database includes information for 24 states.[12] The data necessary to perform my analysis is not currently available for other states. Measures of property wealth are predominantly made up of residential and commercial real property but may also include other types of property (e.g. automobiles or mineral resources).

[11] Property wealth data is not available in each state and year. See Appendix A.2 for a description of data sources and availability for each state.

[12] The states in the database are Arkansas, Connecticut, Florida, Georgia, Idaho, Illinois, Iowa, Kansas, Kentucky, Massachusetts, Minnesota, Mississippi, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Texas, and Washington.

I digitized the raw data based on state records and converted the property wealth measures to total market value of property within the district, wherever possible.[13] I then merged these state records with school district information from the CCD. I most frequently matched on district name, but in some cases I used unique identifiers consistent between the state and CCD records when they were available. See Appendix A.2 for a full description of data sources and steps taken to create the database.

School Finance Formulas

I compile information about school finance formulas from multiple sources. U.S. Department of Education National Center for Education Statistics (2001) provides an overview of each state's funding formula as of the 1998-1999 school year, which provides a useful starting point. I supplement these descriptions with additional information from laws and statutes as well as documentation from state Departments of Education.

I do not attempt to capture every factor that influences state funding. Instead, I focus on the bulk of funding that comes from foundation entitlements and parts of state funding that depend on property wealth. The main feature that needs to be reflected in the school finance formulas is the wealth price. This means that the response to a change in property wealth will be correct in terms of direction and relative magnitude, but the scaling will be off to the extent that I have not accounted for all other categorical grants or other components that are unrelated to property

[13] Some states provide enough information for me to match assessment rates for different types of property with the relevant assessed values to back out market values.

wealth. These differences may weaken the power of my simulated instrument but do not invalidate my instrumental variables estimates.

Student Achievement Data

Graduation rate data come from the CCD and test score measures come from the Stanford Education Data Archive (SEDA). Each data source has its own strength and limitations.

Graduation data comes from the CCD completion information at the district level for most years from 1992 to 2010. I calculate graduation rates by taking the number of diplomas awarded in a given year and dividing by the number of students in the cohort expected to graduate that year based on lagged student counts. Thus, the completion rate can be calculated using a number of cohorts such as the number of students in 11th grade the previous year, number of students in 10th grade two years ago, and so on. Specifically,

$$\text{Grad}_t^g = \frac{\text{Diplomas}_t}{\text{Students}_{t-(12-g)}^g}, \tag{1.18}$$

where Grad_t^g is the gth grade cohort graduation rate in year t, Diplomas_t is the number of diplomas awarded, and $\text{Students}_{t-(12-g)}^g$ is the number of students in gth grade $12-g$ years prior to t. There is some year-to-year variation in district coverage. One important example is years 2003 to 2005, when completion information was only recorded for school districts serving more than 1000 students.[14] My preferred estimates only include districts with data from 2003 to 2005, but results are not sensitive

[14]The number of diplomas awarded was not reported from 2003 to 2005. For these years, I use the reported dropout rate and the base number of students to calculate a measure of diplomas awarded that is consistent with the other years.

to this restriction. It is important to note that this measure is only a proxy for the graduation rate. This measure will also pick up changes in student composition that occur between the year the cohort is measured and when the number of diplomas is measured. It also does not account for students who receive a GED or transfer to a different school district.

The SEDA is a collection of academic achievement, achievement gaps, and school and neighborhood economic and racial composition at various levels of aggregation. The SEDA includes a comprehensive database of district-level test scores for school years 2009 to 2013. The basis for these measures are state standardized tests, which are then adjusted based on comparing the distributions of those tests with the NAEP.[15] For a subset of large, diverse districts, there are also measures of the average gap between white students and black students. These test score measures are reported on the scale of NAEP scores, but I standardize these at the grade-subject level based on the mean and standard deviation in 2009. After 2009 I allow the mean and standard deviation to evolve as the distribution of achievement shifts over time. One of the strengths of the SEDA test score measure is that it covers over 80 percent of districts in the United States. The second key strength is that the measures are comparable across time and geography, which allows me to do this district-level, nationwide analysis. The primary limitation of these data is that they are currently only available for a limited number of years.

[15] See Reardon and Kalogrides (2017) for a full discussion of how these measures are constructed.

Other State and District Controls

Other data used in my analysis include median household income and additional measures used in school finance formulas. The median household income for each school district comes from the 2000 Census and the American Community Survey (ACS) 5-year estimates. These sources provide an estimate of district income for 1999 and then 2009 onward. To account for district-level changes in income, particularly during the great recession, I impute values linearly between the district value in 1999 and the value in 2009. This captures the potential drop in incomes in areas most deeply affected by the recession. Some school finance formulas include a measure of the cost of living to adjust for within state differences in the cost of teacher salaries.[16]

1.3.2 Creating a Balanced Panel of School Districts

Over time, new school districts are formed, old districts are absorbed into existing districts, and some local districts are consolidated into regional districts that serve a larger geographic area. This regional consolidation is especially apparent in the Midwest, where small, rural districts have been combining with greater frequency (Gordon and Knight, 2009). To create a balanced panel of school districts, I combine all districts that are ever associated with each other. For example, Figure A.1 shows the boundaries of two school districts in Minnesota, Brewster and Round Lake. These two districts consolidated into Brewster-Round Lake Public Schools in 2014. Therefore, I treat these two school districts as a single district across the entire analysis period. I sum the property values and student counts in these two districts

[16] These additional variables are outlined in Appendix A.2.

and average the median income and test scores.

I aggregate school districts for two additional reasons: regional district overlap and availability of property value data. Some states have municipality-level elementary districts and regional high school districts that serve multiple municipalities. Even if I have the property values for each municipality, it is not possible to distinguish how the change in property values for a municipality affects each district separately. Figure A.2 illustrates this issue with three municipalities in New Jersey. Bellmawr, Runnemede, and Gloucester municipalities each provide for their own elementary services, and Black Horse Regional High provides secondary services for all three. In both situations, I combine school districts to the lowest level for which I have data and use the aggregated school district in my analysis. While most property value data is reported at the school district level, in some states the data are at the municipality or county level and it is not possible to perfectly map property values to school districts. In these cases, I aggregate districts to the level at which property values are available.

The number of school districts in my sample of 24 states starts at 8,061 in 1999 and falls to 7,649 by 2014 due to actual consolidations. After making my additional district consolidations due to data limitations, my balanced panel consists of 6,500 districts.[17] I also make several exclusions to reduce noise and volatility in my per-pupil measures, which are similar to the exclusions in Lafortune, Rothstein, and Schanzenbach (2018). Specifically, I remove districts with fewer than 100 students at any time and district-year observations with enrollment: more than double the

[17] I perform additional analyses to show my results are robust to dropping all consolidated districts. These results are available in Appendix Table A.20.

district's mean enrollment, more than 15 percent different from enrollment in either adjacent year, or more than 10 percentage points above or below the district's average growth in enrollment. I also remove district-year observations with per-pupil expenditure or simulated revenue more than 5 times larger or smaller than the state average. Together, these restrictions affect roughly 19.5% of district-year observations, but many of these districts are also dropped due to other missing data. My main conclusions are not sensitive to these restrictions.[18]

1.3.3 Summary Statistics

Summary statistics for my main estimation samples for graduation rates or SEDA test scores are presented in Table 1.1. The table highlights the fact that each outcome is available for distinct subsets of the data. The first two columns present means and standard deviations for the graduation rate sample, where spending information is either available for at least 1 year prior to cohort graduation and for at least 4 years prior. The last column reports statistics for the sample with SEDA test scores. The graduation rate samples cover just under 3,000 school districts, while the test score sample has just under 6,000 school districts. The average graduation rate is about 80 percent for the 10th grade cohort.

My sample consists of districts from 24 states. Table 1.2 compares the characteristics of districts in states including in my sample and those that are not included, to speak to the external validity of my estimates. These differences are calculated as of 2009. The first column shows statistics for the districts in states in my sample,

[18]Estimates without these sample restrictions are available in Appendix Table A.20.

column (2) provides statistics for districts in states not in my sample, column (3) displays the p-value of the difference between column (1) and (2), and the final column is statistics for all districts. The 24 states in my sample account for 55 percent of all students and 56 percent of districts. Districts are similar in their average number of students and teachers, and in income. There are some differences in property tax revenue per student and fractions of students eligible for free or reduced price lunch, in special education or who are a racial minority. However, these differences are small and provide suggestive evidence that my estimates for the relationship between spending and student outcomes would generalize to other states not in my sample.

1.4 Method

The central challenge in estimating the causal effect of total spending on student achievement is endogeneity between expenditures and student outcomes. For example, districts with a higher number or percentage of children who come from low-income families receive additional funding through programs such as Title I. This negatively biases cross-sectional estimates. On the other hand, districts with a higher fraction of parents that are high income or are more engaged in education also receive greater resources through a higher willingness to pay taxes for education and potentially donations to the district. This situation instead causes a positive bias in a cross-section. These are just two examples of the bias that comes from factors related to both educational outcomes and levels of spending. It is unclear which type of bias dominates in any given sample, so cross-sectional OLS estimates are difficult to interpret in a causal manner. Controlling for fixed differences between districts accounts for many of these cross-sectional biases, but changes in student or family characteristics also introduce bias.

1.4.1 Simulated Instrument

To address these endogeneity concerns, I construct an instrument that captures the mechanical response in revenue to changes in property values through fixed school funding formulas. States regularly change the funding level per student to address student needs and changing costs of education. Districts also frequently change their tax rates based on their budgetary needs and the current level of property wealth. Both of these policy decisions are likely to be endogenously related to changes in student performance. In my instrument, I fix both state funding formulas and district property tax rates in a base year. The only determinant of funding left to vary is property wealth. With fixed tax rules, increased property wealth leads to increased property tax revenue and, often, decreased state transfers.

Specifically, I start with a district's base year effective tax rate, which is given by

$$ETR_0^d = \frac{\text{Property Tax Revenue}_0^d}{\ell_0^d W_0^d}, \tag{1.19}$$

where Property Tax Revenue$_0^d$ is the total revenue from property taxes in the base year and W_0^d is the total market value of property in the base year.[19] Previous

[19]Due to data limitations, I am unable to recover market values from assessed values for all districts. The instrument exhibits the same variation for districts with assessed, rather than market, values but the magnitude of the first stage will be scaled by the portion of ℓ_0^d for which I am not able to account.

research finds that property values are determined, in part, by the quality of schools in the area (Oates, 1969; Black, 1999; Bayer, Ferreira, and McMillan, 2007; Ries and Somerville, 2010). Thus, student achievement may directly affect property values in the district. To avoid this simultaneity issue, I calculate simulated property wealth in year t as

$$\tilde{W}_t^d = \frac{W_t^s - W_0^s}{W_0^s} \times W_0^d, \tag{1.20}$$

where W_t^s and W_0^s are state-level property wealth in year t and the base year, respectively. I use state-level changes that omit the focal district to remove any potential impact of district-level changes on the aggregate. This can also be done at other levels of aggregation (e.g. CBSA or national). The higher the level of aggregation, the less concern about characteristics of the district impacting property values.[20] My measure of simulated property wealth is a Bartik-style shift-share measure, where the share is the baseline level of property wealth and the shift is changes at the state-level (Bartik, 1991).

Simulated local revenue, $\tilde{L}_t^d$ is then the base year effective tax rate times simulated wealth, or

$$\tilde{L}_t^d = ETR_0^d \times \ell_t^d \tilde{W}_t^d. \tag{1.21}$$

The effective tax rate absorbs most of the L_t^d function by accounting for assessment rates, delinquency rates, and exemptions. Simulated state revenue, $\tilde{S}_t^d$, is calculated by substituting a combination of the base year statutory tax rate, τ_0^d, base year effective tax rate, ETR_0^d, base year student counts, and current year simulated property

[20] I perform additional analyses with simulated revenue calculated using national changes in wealth. These analyses are available in Table A.20 and provide similar results to my leave-one-out measure for graduation rates, but the first stage becomes weak for the test score samples.

wealth into the base year state funding formula. That is,

$$\tilde{S}_t^d = S_0(\tau_0^d, \tilde{W}_t^d, wADM_0^d, \tilde{L}_t^d). \tag{1.22}$$

Here, S_0 captures important characteristics of funding formulas in the base year that determine the response in state revenue to changes in property values. The set of variables included in simulated state revenue depend on the particular state funding formula. To explain how I construct simulated state revenue, consider the examples of New Mexico and Georgia. In New Mexico, the foundation amount was \$2,344.09 per weighted pupil in 1999 and the only other variables state funding depends on are student counts and property wealth. Thus, simulated state revenue for New Mexico is calculated as

$$\tilde{S}_t^d = \$2,344.09 \times wADM_0^d - \ell_t^d \times 0.0005 \times \tilde{W}_t^d, \tag{1.23}$$

and it follows that simulated revenue is:

$$\tilde{R}_t^d = \$2,344.09 \times wADM_0^d + (ETR_0^d - \ell_0^d \times 0.0005) \times \tilde{W}_t^d. \tag{1.24}$$

For Georgia, the foundation amount in 1999 was \$2,038.74 per weighted pupil, so simulated state revenue is

$$\begin{aligned}\tilde{S}_t^d &= \$2,039 \times wADM_0^d - \ell_0^d \times 0.005 \times \tilde{W}_t^d \\ &+ \begin{cases} 0.00325 \times \left(W_0^{90} - \ell_0^d \tilde{W}_t^d\right) & \text{if } \ell_0^d \tilde{W}_t^d < W_0^{90} \text{ and } \tau_0^d > \frac{8.25}{1000} \\ (\tau_0^d - 0.005) \times \left(W_0^{90} - \ell_0^d \tilde{W}_t^d\right) & \text{if } \ell_0^d \tilde{W}_t^d < W_0^{90} \text{ and } \frac{5}{1000} \le \tau_0^d \le \frac{8.25}{1000} \\ 0 & \text{if } \ell_0^d \tilde{W}_t^d \ge W_0^{90} \end{cases}\end{aligned} \tag{1.25}$$

and simulated revenue is

$$\tilde{R}_t^d = \$2,039 \times wADM_0^d + (ETR_0^d - \ell_0^d \times 0.005) \times \tilde{W}_t^d$$
$$+ \begin{cases} 0.00325 \times \left(W_0^{90} - \ell_0^d \tilde{W}_t^d\right) & \text{if } \ell_0^d \tilde{W}_t^d < W_0^{90} \text{ and } \tau_0^d > \frac{8.25}{1000} \\ (\tau_0^d - 0.005) \times \left(W_0^{90} - \ell_0^d \tilde{W}_t^d\right) & \text{if } \ell_0^d \tilde{W}_t^d < W_0^{90} \text{ and } \frac{5}{1000} \leq \tau_0^d \leq \frac{8.25}{1000} \\ 0 & \text{if } \ell_0^d \tilde{W}_t^d \geq W_0^{90} \end{cases} . \tag{1.26}$$

This same procedure is carried out for each district in my sample.

1.4.2 Empirical Strategy

I estimate two-stage least squares (2SLS) models relating student achievement to spending, using simulated revenue as an instrument for actual spending. The first stage equation is:

$$\text{Spending}_{d,t-\tau} = \alpha_0 + \alpha_1 \tilde{R}_{d,t-\tau} + \alpha_2 W_{d,t} + \boldsymbol{X}_{d,t} \boldsymbol{\alpha}_3 + \gamma_d + \gamma_{s,t} + \eta_{d,t} \tag{1.27}$$

where $\text{Spending}_{d,t-\tau}$ is observed log spending in district d in the τ years before calendar year t. This can either be the values of simulated revenue and spending τ years ago or the average over the past τ years. $W_{d,t}$ is the value of property in the district, $\mathbf{X}_{d,t}$ is a vector of district characteristics including log number of students, median household income, fraction of students with an IEP, fraction of student eligible for free or reduced price lunch, fraction of black student, and fraction of Hispanic students. District fixed effects are given by γ_d and state-by-year fixed effects are given by $\gamma_{s,t}$, where s indicates the state in which district d is located.

The second stage is:

$$A_{d,t} = \beta_0 + \beta_1 \widehat{\text{Spending}}_{d,t-\tau} + \beta_2 W_{d,t} + \boldsymbol{X}_{d,t}\boldsymbol{\beta}_3 + \delta_d + \delta_{s,t} + \varepsilon_{d,t}, \qquad (1.28)$$

where $A_{d,t}$ is district-level student achievement, $\widehat{\text{Spending}}_{d,t-\tau}$ is predicted spending over the past τ years from the first stage, and other measures are as described in the first stage. In both equations, standard errors are clustered at the district level.

Education is a cumulative process, so even if student achievement responds directly to education spending, it is unlikely to do so in the same year. Instead of measuring the immediate effect of spending on contemporaneous test scores, I consider current and lagged district spending individually and on average. Ideally, I would examine the effect of spending over the past four years on fourth grade test scores and spending over the past eight years on eighth grade test scores. In practice, my instrument is stronger nearer to the base year, so I restrict my attention to the lags with a strong first stage. Since graduation rates are available in earlier years (1999-2010) my first stage is strong in closer relative years, lower values of τ. Thus, for models of the graduation rate I focus on spending between the current year ($\tau = 0$) and four years prior ($\tau = 4$). Test score measures are only available for later years (2009-2013) and therefore have a strong first stage with longer lags, higher values of τ. For test score outcomes I focus on spending in the five to eight years prior to the year the outcomes are measured. These are due to my empirical approach and do not necessarily reflect an underlying aspect of the education production function.

1.4.3 Identification

Including observed district property values in my regressions makes clear that my model is not identified by within-district variation in property wealth. Instead, identifying variation comes from the interaction of property wealth and the fixed tax rules. In this case, the exclusion restriction is that simulated revenue is only related to student achievement through its affect on spending. Since the simulated instrument is determined only by base-year tax rules and adjustments to base-year property wealth, the exclusion restriction is violated if changes in unobserved factors related to changes in student outcomes (such as demographic shifts) are also related to the interaction of base-year tax rates and base-year property wealth. Thus, simulated revenue should not be related to large changes in demographics. The exclusion restriction would also be violated if demographic trends were determined by the combination of initial tax rates and property wealth.

In order to assess the validity of my empirical strategy, I propose several exercises that show whether the data is consistent with the assumptions necessary for my estimates to reflect a causal effect. First, in Figure 1.4, I explore whether the data support the exclusion restriction by plotting trends in district characteristics separately for four subgroups. The subgroups are created by splitting the sample by districts above and below the median for initial property wealth and effective tax price. The exclusion restriction would be violated if changes in district characteristics related to student achievement are related to baseline tax rates and property wealth. To provide context, the top two figures show the trends in property values and simulated revenue per pupil across the four subgroups. Property values exhibit the same upward trend for each group until 2009, when values in districts with high initial wealth decreased and values in districts with low initial property wealth stopped increasing. The trends in simulated revenue are similar until about 2003 when districts with low initial wealth have the largest increases. These lines are expected to diverge to the extent that there is variation in baseline tax rates and property wealth that are relevant for differences in revenue. The fraction of students eligible for free or reduced-price lunch trends up similarly for all four subgroups, which suggests no differential trends in district poverty. Finally, the fraction of students who are black increases in districts with high initial property wealth and remains relatively stable for districts with low initial property wealth. This suggests that my estimates may be attenuated because the districts with the largest growth in fraction black are also the districts with the largest increase in simulated revenue per student. Taken together, these provide evidence that my estimates are not being driven by trends in student characteristics.

In the next section I show two additional checks for the validity of my research design. First, I estimate the effect of simulated revenue on various measures of student composition. A strong relationship between student composition and simulated revenue could mean my estimates are biased. I will show that effects are small and the relationships that are significant would work in the opposite direct of the results I find. Second, I do a placebo test of whether future future simulated revenue is related to current outcomes. If my estimates are driven by endogeneity between my measure of revenue and student outcomes then the order of spending and outcomes would not matter. I will show that current and past spending matter, but future spending does not, which provides further evidence in support of my identification strategy.

1.5 Results

I show the first-stage effect of log simulated revenue on log total expenditures for the graduation rate samples and SEDA test score samples in figures, with corresponding tables available in the appendix. The y-axis of the figures are the estimated first-stage coefficient given a 10 percent increase in spending and the x-axis is number of years relative to when the cohort is set to graduate. Coefficients are shown as dots, 95% confidence intervals are shown as whiskers, and F statistics for each estimate are in brackets. Each column of the table correspond to one of the relative years on the x-axis and each panel is one of the samples.

First-stage estimates for the graduation rate samples are shown in Figure 1.5, with corresponding results in Table A.4. The coefficients are mostly between 0.01 and 0.02, which means that a 10 percent increase in simulated revenue increases spending by 1 to 2 percent. This suggests that school districts and state governments respond to the mechanical change in revenue from changes in property values, but not enough to fully counteract the increase in revenue. However, as I previously mentioned, the scaling of simulated revenue makes these an underestimate of the true magnitude. The figures also show a pattern wherein estimates with a short lag (4 years or less) have a strong first stage, while estimates with a longer lag have smaller coefficients that either are not statistically different from zero or have F statistics less than 10.

This is consistent with there being a strong first stage near the base year of 1999 that becomes weaker the further away the measure is from the base year.

Similar estimates for the SEDA test score samples are reported in Figure 1.6 and Table A.5. The coefficients are centered around 0.02 for the later lags and decline to be around 0.01 for the earlier lags. These effects are similar to the magnitude of those in the graduation rate samples, but show a pattern that is opposite of the graduation rate samples, with stronger estimates for the longer lags and estimates that are attenuated and have F statistics below 10 for lags fewer than 3 years.

Simulated revenue is a strong instrument near the base year of 1999, but becomes weak farther away from the base year. This pattern is not due to actual heterogeneity in the lag structure, but is driven by the calculation of the instrument. Graduation rates are measured from the base year until 2010, but test scores are measured from 2009 to 2013. Thus, the short lags in the graduation sample and the long lags in the SEDA test score sample are strong because they come from the years in which the simulated instrument is strong. Since my 2SLS results are only reliable when the first stage is strong, I focus on spending in the 1 and 4 years before graduation rates are measured and spending 5 to 8 years prior to when test scores are measured.

Table 1.3 reports estimates with various averages of the prior years of simulated revenue and spending. Column (1) is the average of the current year and the previous year, column (2) is the average of the current year and the previous 4 years, estimates with the average of this year and the past 8 years are shown in column (3), and the last column has estimates averaged from 5 to 8 years prior to when the outcome is measured. Panels A through D present estimates for the graduation rate samples and panel E shows results for the SEDA test score sample. The estimates using average lags are consistent with the individual lags in the pattern of first-stage strength. The first stage is strong for averages of 1, 4, and 8 lags for graduation sample, but not for the average of 5 to 8 years prior. The SEDA test score sample has a strong first stage for the 8-year lag and the 5-8 year average, but not for the 1 and 4 year lags.

The results of my 2SLS analysis are reported in individual lags as both figures and tables and average lags in a table similar to the first stage results. Instead of showing all the individual lags, I only report the results for the lags that have a strong first stage. Figure 1.7 shows the individual lag results for graduation rates, with corresponding estimates in Table A.4.[21] The coefficients are positive and significant in the year of and before graduation, but smaller and not statistically significant 2 to 4 year prior. The estimates in Table 1.4 suggest that the average effect of a 10 percent increase in spending on graduation rates ranges from 2.1 to 4.4 percentage points. These results suggest that increased spending is most effective at improving graduation rates for those near graduation.

Figure 1.8 and Table A.7 report the 2SLS results of spending on SEDA test scores. The coefficients are generally positive, significant, and around 0.1 standard deviations in magnitude. The exception is for 8th grade math scores, which are similar in magnitude but vary from a point estimate near zero for 8 years prior up to 0.2 for 4 years prior. The average lag results in Table 1.5 suggest that increasing spending by 10 percent in the 5 to 8 years prior to the test increase 4th grade math scores by 0.078 standard deviations, 4th grade reading scores by 0.088 standard deviations,

[21] The coefficients for other individual lags with a weak first stage are imprecisely estimated and generally not informative, but are available upon request.

8th grade math scores by 0.048 standard deviations, and 8th grade reading scores by 0.093 standard deviations. It is important to note that increased spending has a lasting impact on test scores, and improvements made before students enter school have a significant effect several years later.

These estimates are consistent with the most recent, well-identified estimates for the effect of spending on test scores. In particular, Lafortune, Rothstein, and Schanzenbach (2018) find that after 10 years of increased spending by \$1,000 per pupil, due to school finance reforms, test scores increased between 0.12 and 0.24 standard deviations. Other studies find positive effects of spending on test scores in single-state case studies (Guryan, 2001; Papke, 2005). My estimates suggest that a thousand dollar increase in spending per pupil results in a 0.051 to 0.066 standard deviation increase in test scores (In my sample, average spending per pupil is \$13,719.24, so \$1,000 is a 7.29 percent increase. Scaling my estimates by 0.729 gives $0.09 \times 0.729 = 0.066$ for 4th grade test scores and $0.07 \times 0.729 = 0.051$ for 8th grade reading scores.), which is smaller than Lafortune, Rothstein, and Schanzenbach (2018). However, the parameter I estimate is the effect of increased spending 5 to 8 years before the test is taken, while Lafortune, Rothstein, and Schanzenbach (2018) report the effect of a persistent increase in spending over the previous 10 years. If there is a cumulative effect of being in a district with more resources, then scaling my estimates to 10 years rather than 4 years provides effect sizes consistent with their study.

1.5.1 Validity Checks

If my measure of spending is correlated with changes in the types of students in the district, then the estimates could reflect changes in student composition rather than changes in student achievement. I explore whether this is the case in Table 1.6, which shows the effect of spending in the current and previous year in the graduation rate sample in the first 5 columns and average spending 5 to 8 years prior in the test score sample in the last 5 columns. The first column shows estimates for the log number of students and columns (2) through (5) show estimates for the fraction of students in different categories including fraction black, fraction Hispanic, fraction with an IEP (special education), and fraction eligible for free or reduced price lunch. A 10 percent increase in spending increases the number of students by 232 in the graduation rate sample and 210 in the test score sample, which amounts to a 4.5 percent increase. In the graduation rate sample, the fraction of students who are black increased by 0.22 percentage points, fraction Hispanic increased by 1.28 percentage points, fraction of students with an IEP increased by 0.23 percentage points, and fraction of students eligible for free or reduced-price lunch decreased by 0.64 percentage points. However, the decrease in free or reduced-price lunch eligibility is not statistically significant. The SEDA test score sample shows a similar increase in the fraction Hispanic, but the estimates for fraction black, fraction with an IEP, and fraction eligible for free or reduced price lunch are smaller in magnitude and not statistically different from zero. While several of these coefficients are statistically significant, they are relatively small in magnitude and rule out large changes in student composition driving my results. In fact, the small changes are generally in the direction that would work against finding an effect if they were true shifts in the district population. These changes are also consistent with retaining more students that are most in danger of dropping out.

As a falsification test, I also estimate the effect of spending over several following years on outcomes in the current year. Table 1.7 shows the relationship between average spending over the following four years on graduation rates in the current year. The first stage is strong, but the 2SLS estimate is small, negative, and not statistically significant, which provides additional evidence that my estimates reflect a causal effect of spending on student achievement. I am unable to do a similar falsification test for test scores because I do not have a strong first stage for spending in any years following a test-score measure.

1.5.2 Exploring Mechanisms and Heterogeneity

Although my instrument only allows me to estimate the causal effect of total resources, it is instructive to examine the categories in which districts choose to spend their extra funds. Table 1.8 shows 2SLS estimates for the relationship between expenditures and local, state, and federal revenue. All measures are in thousands of real 2013 dollars per pupil. The first three columns show results for the 10th grade cohort graduation sample with average lags over the current and 1 previous year, while the last three columns present estimates for the SEDA test score sample with average lags 5 to 8 years prior to the test. The majority of increased revenue comes through local sources. State aid also increases, but the estimate for the SEDA test score sample is negative and less precisely estimated. Table 1.9 and Table 1.10 report

2SLS estimates of total expenditures on mutually exclusive and collectively exhaustive subcategories of spending for the graduate rate sample and test score sample, respectively. These estimates suggest that the majority of increased spending was devoted to current expenditures, capital outlay, and payments to other organizations. The larger than average payments to the state, other schools, and private schools are consistent with districts bearing a portion of the responsibility of students who would otherwise attend but are attending other schools. Table 1.11 breaks up current expenditure into instructional, support service, and other categories. This shows that the majority of current expenditures are instructional expenditures, but support services also receive a significant portion of the funds.[22] The difference between the samples in the fraction of each dollar going to current expenditures is driven by less support service spending in the test score sample.

I also explore heterogeneity in the effect of spending on graduation rates. In Table 1.12 I show results for models fully interacted with an indicator that equals 1 in periods that simulated revenue decreased from the previous period in panel A. The coefficients on log spending represent the effect of increased spending and the coefficient on loss interacted with log spending shows how much larger or smaller the effect of spending is when spending decreases. In these models I instrument for the two spending variables with simulated revenue and simulated revenue interacted with the indicator for a loss. Because I have more than one instrument, I report an F statistic suggested by Kleibergen and Paap (2006) as a test for the strength of the

[22] Additional tables with estimates for each subcategory of spending are available in Online Appendix A.

instruments and find they are reasonably strong.[23] The coefficients on log spending for graduation rates are similar in magnitude to the non-interacted coefficients in Table 1.4 and the coefficients on the interaction term are small and only statistically different from zero for the 11th grade graduation cohort. This estimate suggests that a 10 percent increase in spending increases the number of diplomas per 11th-graders (1 year ago) 0.31 percentage more when spending decreases than when spending increases. That represents is a 15.7 percent larger magnitude effect when budgets are cut than when they expand. I consider this merely suggestive because gains are only significantly different from losses for the 11th grade cohort measure, and the direction of the effect is not consistent nor significantly different from zero for test scores.

Panel B shows the results of similar analyses with models fully interacted with an indicator equal to 1 if the district has median household income below the median in their state. The estimates for graduation rates are larger in magnitude for high-income districts than the average across all districts from the baseline model in Table 1.4. My estimates suggest that increasing spending by 10 percent increases graduation rates by 4.32 to 7.1 percentage points in high income districts and 0.3 to 1.45 percentage points in low income districts. The difference between high and low income districts is statistically significant for all cohorts.

[23] The Kleibergin-Paap statistic is a generalization of the statistic suggested by Cragg and Donald (1993) for cases with non-i.i.d. standard errors.

1.6 Conclusion

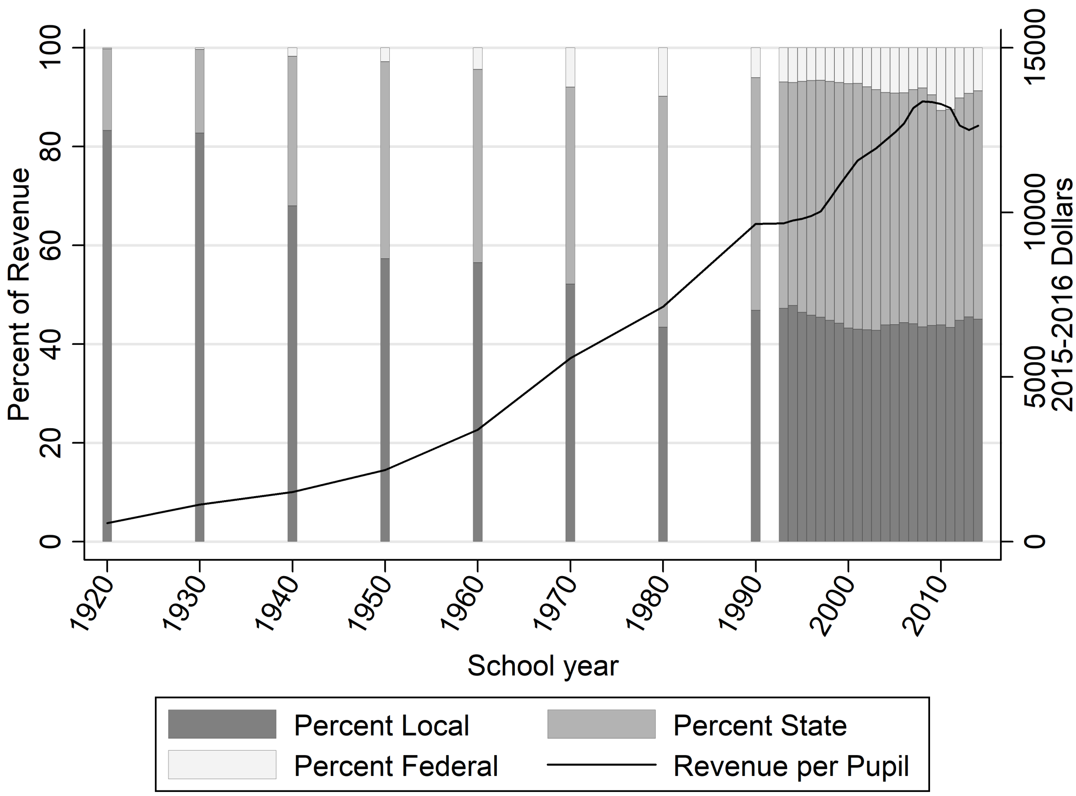
This paper addresses the question of whether money spent on education affects graduation rates and test scores using the interaction of market changes in property values with fixed school finance rules as an instrument for spending. I find that a 10 percent increase in spending increases graduation rates by 2.1 to 4.4 percentage points. A 10 percent increase in spending also increases 4th and 8th grade math and reading scores by between 0.05 and 0.09 standard deviations. Increased spending primarily goes to current expenditures, new construction, and payments to other organizations such as the state government and local private schools. The improvement in graduation rates is observed almost entirely in high-income districts. Spending has lasting effects on test scores, so that students benefit from investments made before they even begin school.

In sum, the answer to the question of whether money matters in education is yes. Further understanding the way in which money matters will also help shape efficient policies. For example, the reduced form relationship I find between property values and student outcomes is important to consider when crafting school finance plans. If formulas provide additional funding to districts with low levels of property wealth, but reinforce this relationship, it could increase spending volatility enough to offset the benefits of increased funds. Another implication of my finding is that we have not yet reached the flat of the curve and marginal increases in spending still result in meaningful improvements in the quality and quantity of education. Also, because I find that increased spending before a student even enters school significantly improves their test scores, estimates that relate contemporaneous expenditures and test scores will likely miss the true impact of the spending.

The relationship between property values and local revenue is not unique to school finance. Thus, my approach can be applied directly to other locally-financed public programs. This is especially useful in other cases where it is difficult to measure the effect of resources on outcomes because of the relationship between the outcome and the level of investment, such as the number of police officers and the level of crime. While other contexts do not have the same type of equalization schemes as seen in school finance, other state-level limitations on local taxing behavior provide between-state variation in wealth prices.

1.7 Figures & Tables

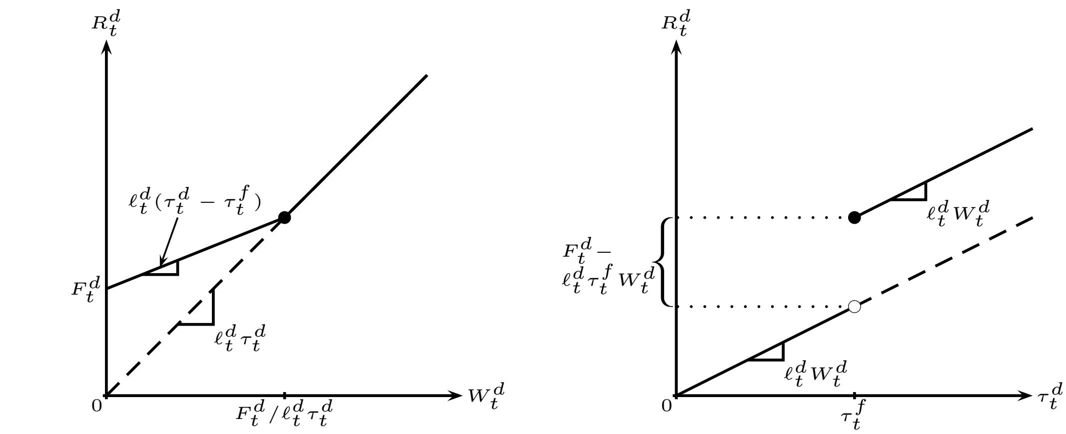
Figure 1.1: Historical Sources of School District Revenue



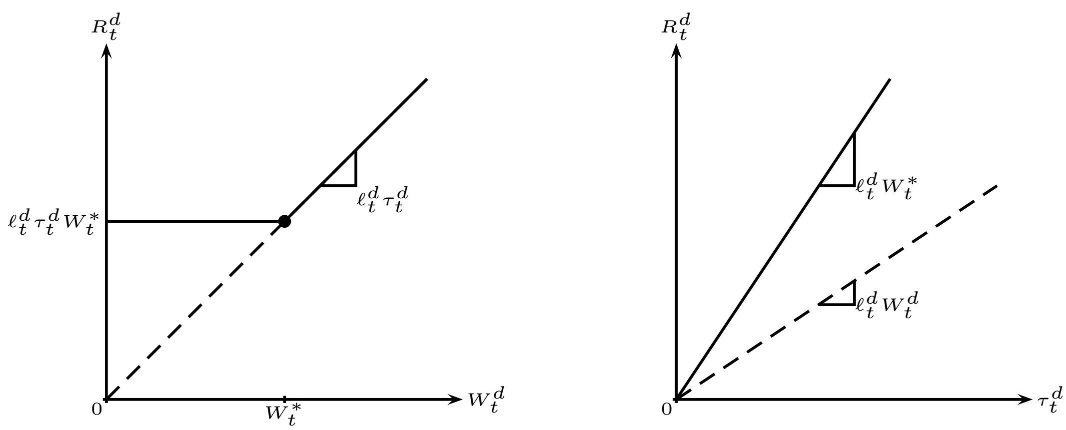
Notes: Data from U.S. Department of Education, National Center for Education Statistics, Biennial Survey of Education in the United States, 1919-20 through 1949-50; Statistics of State School Systems, 1959-60 and 1969-70; Revenues and Expenditures for Public Elementary and Secondary Education, 1979-80; and Common Core of Data (CCD), "National Public Education Financial Survey," 1989-90 through 2013-14.

Figure 1.2: Relationship between revenue and wealth/tax rate for general school finance plans

A. Foundation

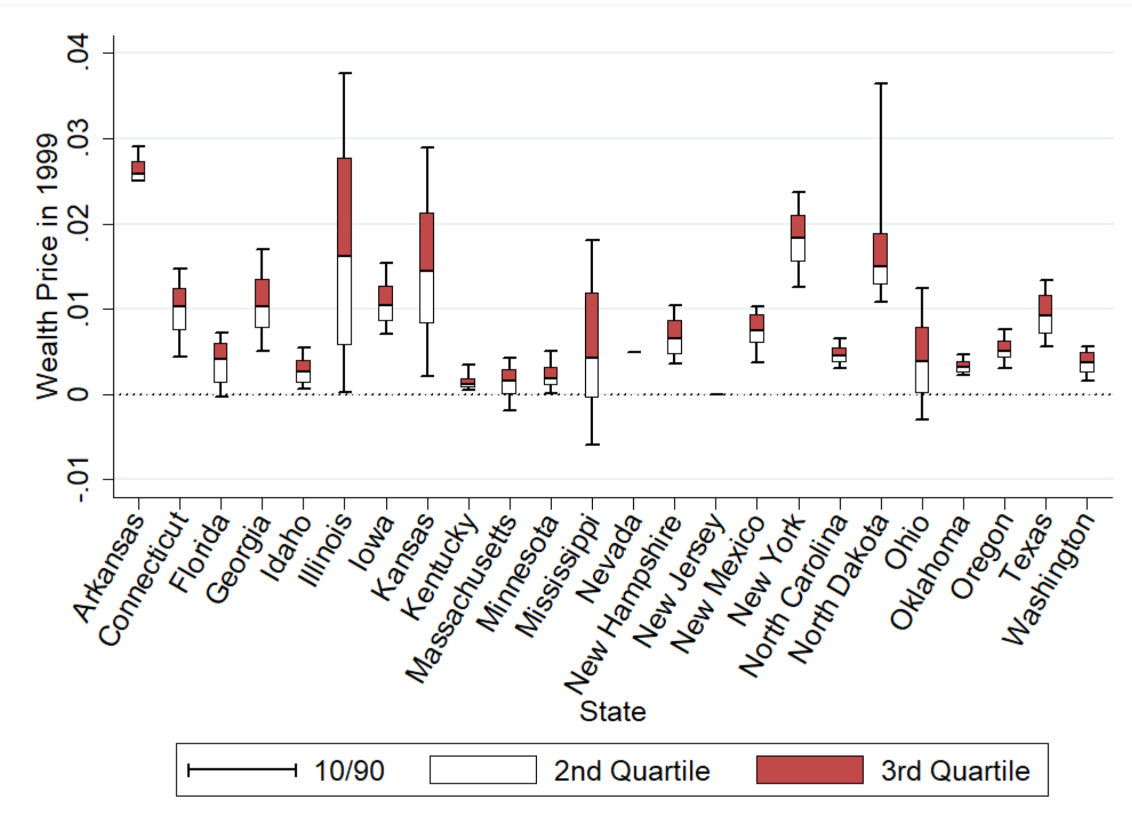


B. District Power Equalization



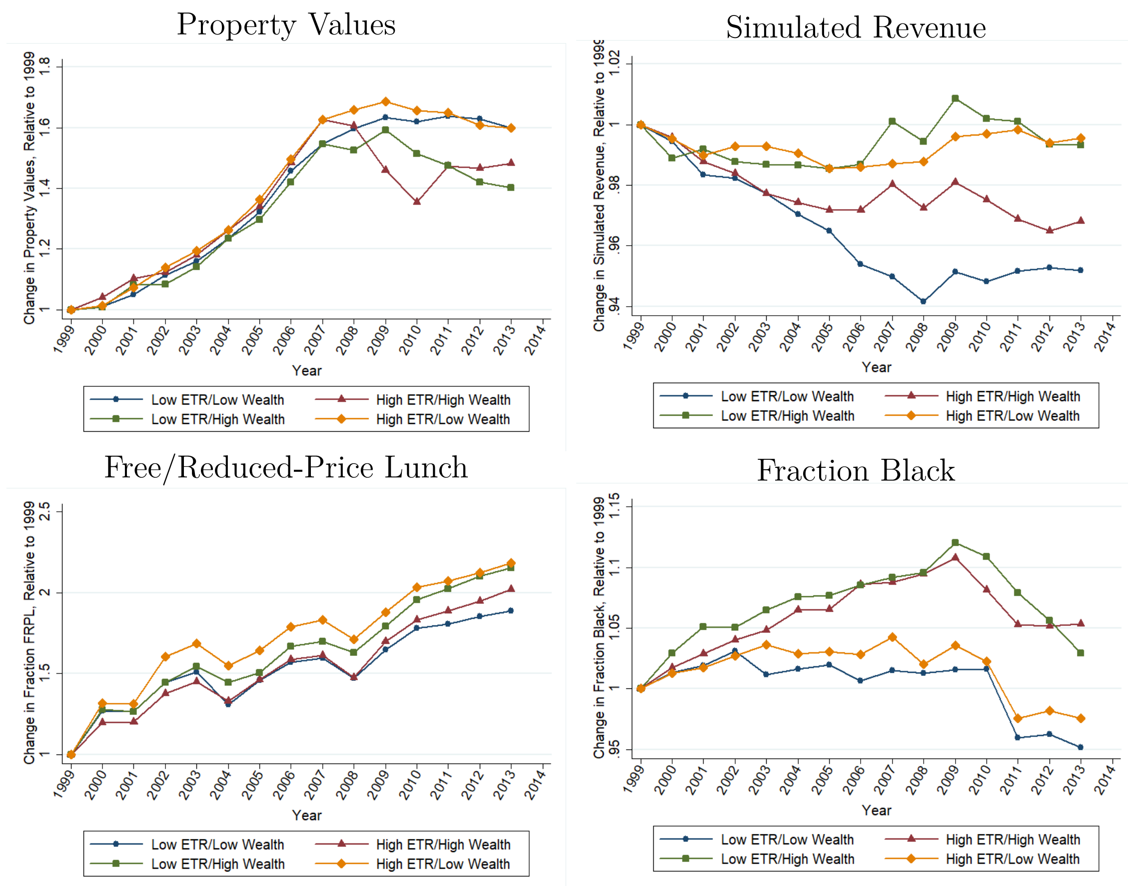
Notes: Panel A shows the relationship between revenue and property wealth (left) and revenue and the tax rate (right) for a foundation plan with foundation tax rate τ_t^f and guaranteed foundation amount F_t^d. Panel B shows the relationship between revenue and property wealth (left) and revenue and the tax rate (right) for a district power equalization plan with guaranteed yield of W_t^*. Dotted lines represent local revenue with no state aid.

Figure 1.3: Distribution of estimated wealth price in 1999



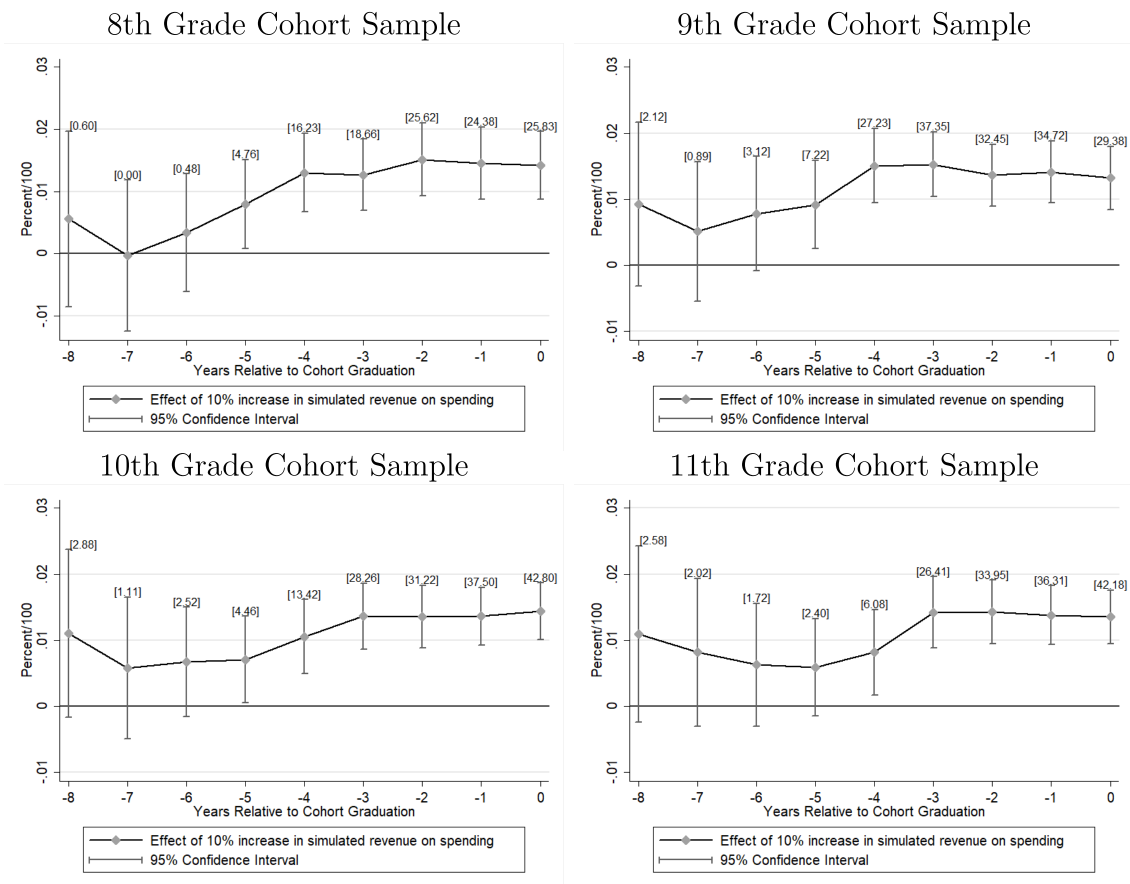
Notes: The wealth price is the fraction of each additional dollar of property wealth that districts take in as revenue. Calculations of the wealth price based on policies in 1999 can be found in Online Appendix B.

Figure 1.4: Change in district characteristics relative to 1999, by base-year wealth and effective tax rate



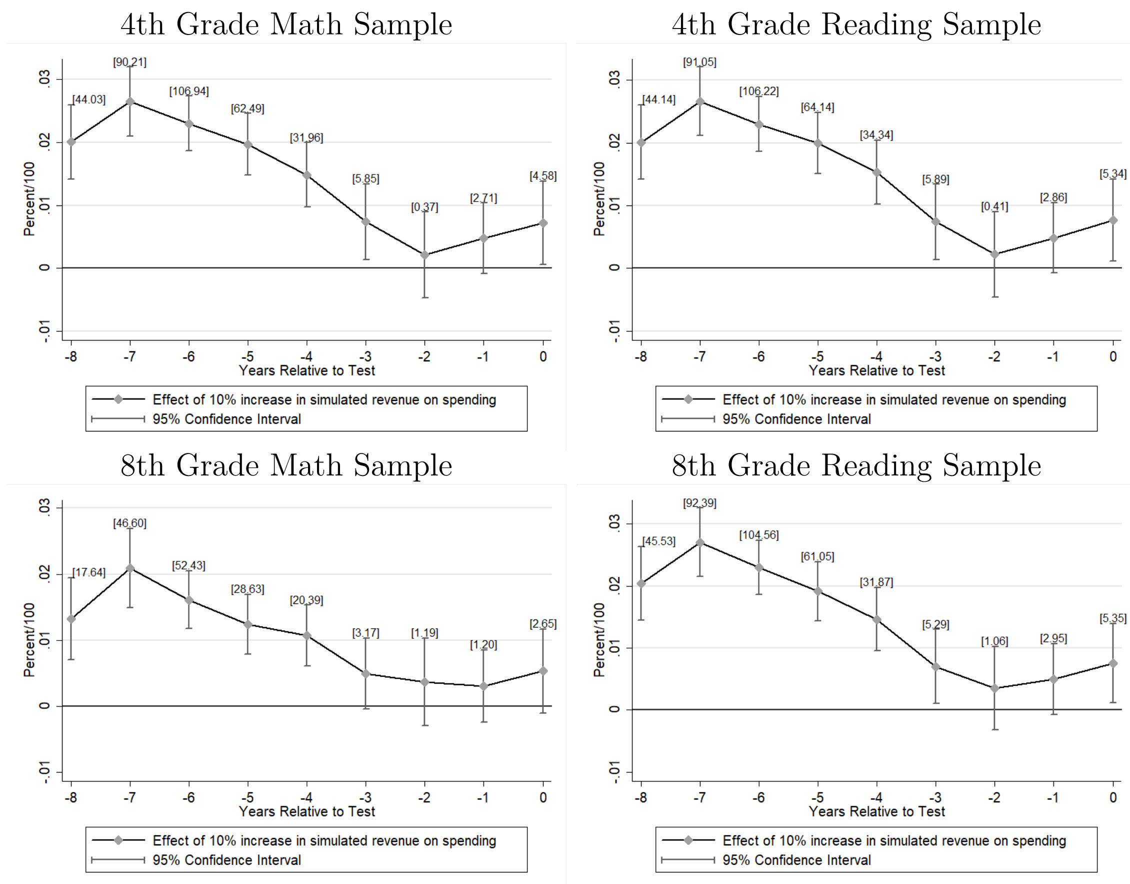
Notes: Figure 1.4 plots the mean change in property values, simulated revenue per pupil, fraction of students eligible for free or reduced price lunch, and fraction of students who are black, relative to 1999, for 4 groups: (1) districts with below median effective tax rate and below median property wealth, (2) districts with above median effective tax rate and above median property wealth, (3) districts with below median effective tax rate and above median property wealth, and (4) districts with above median effective tax rate and below median property wealth. Effective tax rates and property wealth are calculated as of 1999 and medians are calculated at the state level.

Figure 1.5: First-stage effect of a 10% increase in simulated revenue on total expenditure for graduate rate samples – individual year lags



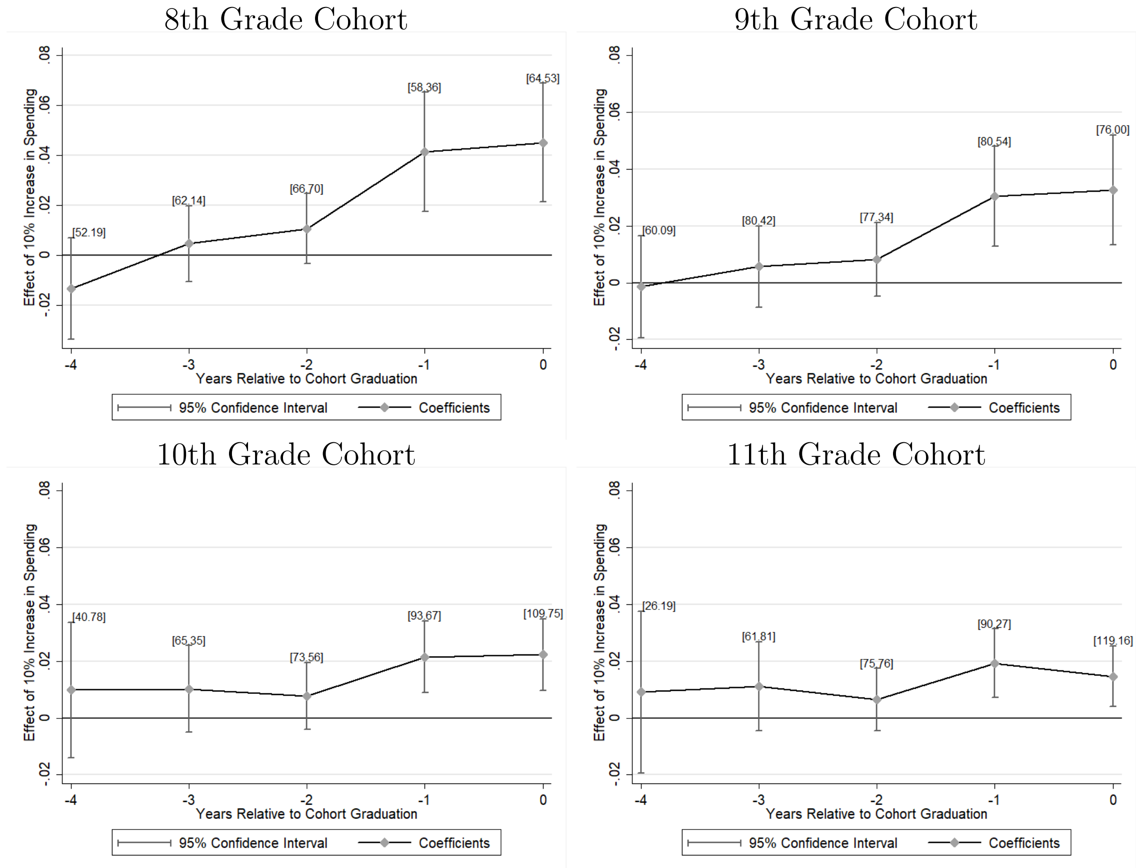
Notes: Figure 1.5 presents point estimates (divided by 10), 95 percent confidence intervals, and F statistics in brackets, from individual regressions of lagged total expenditures on simulated revenue with the same lag for samples with non-missing graduation rates. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level.

Figure 1.6: First-stage effect of a 10% increase in simulated revenue on log expenditure for SEDA test score samples – individual year lags



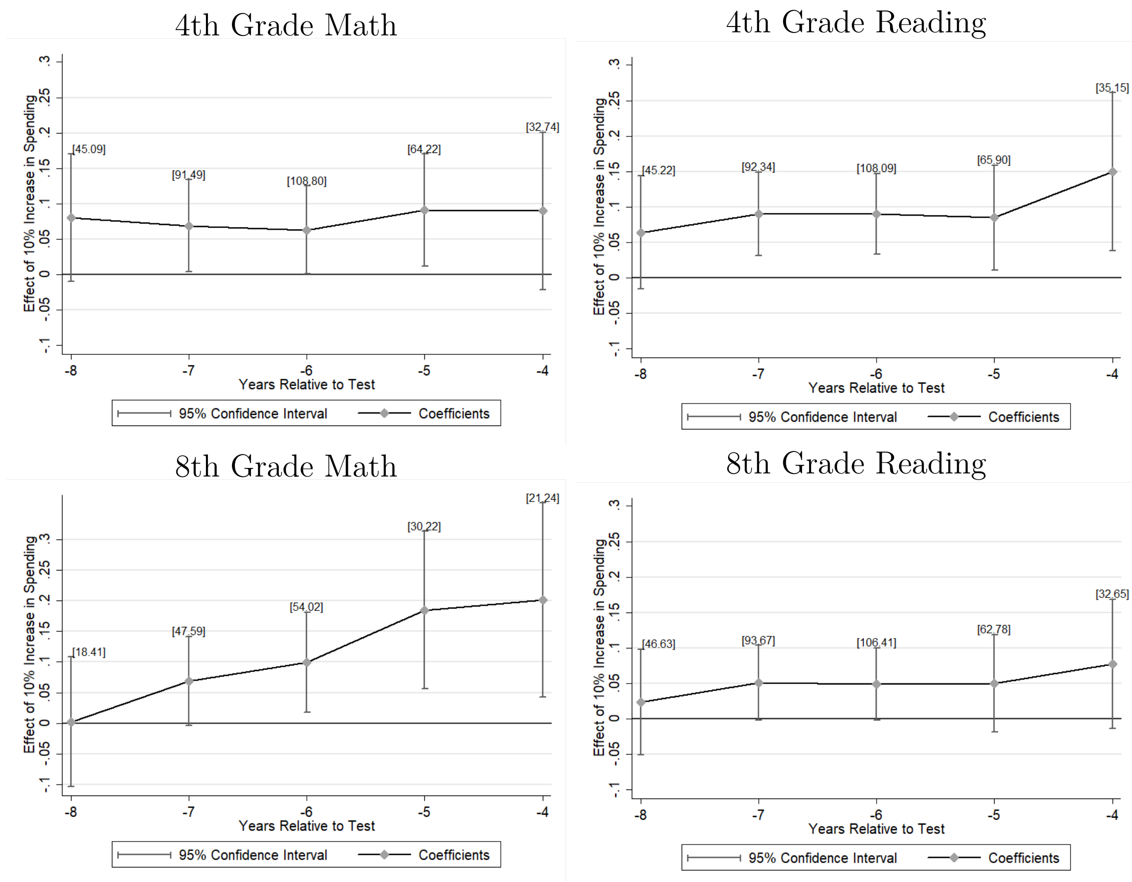
Notes: Figure 1.6 presents point estimates (divided by 10) and 95 percent confidence intervals, and F statistics in brackets, from individual regressions of lagged total expenditures on simulated revenue with the same lag for samples with non-missing test scores. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level.

Figure 1.7: Two-stage least squares estimates of log spending on graduation rates



Notes: Figure 1.7 presents point estimates (divided by 10), 95 percent confidence intervals, and F statistics in brackets, from individual 2SLS regressions of graduation rates on lagged log total expenditures instrumented by lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level.

Figure 1.8: Two-stage least squares estimates of log spending on SEDA test scores



Notes: Figure 1.8 presents point estimates (divided by 10), 95 percent confidence intervals, and F statistics in brackets, from individual 2SLS regressions of test scores on lagged log total expenditures instrumented by lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level.

Table 1.1: Summary statistics for main estimation samples

	(1)	(2)	(3)
	10th Grade Cohort		SEDA Test
	1-year	4-year Lag	Score Sample
Graduation Rate	0.81	0.81	
	(0.17)	(0.19)	
Average Lagged Spending ($1,000 PP)	12.57	12.37	12.63
	(4.29)	(3.94)	(4.32)
Average Lagged Simulated Revenue ($1,000 PP)	7.22	7.08	7.05
	(3.91)	(3.70)	(3.83)
Fraction Special Education	0.13	0.13	0.14
	(0.05)	(0.05)	(0.04)
Fraction Black	0.11	0.10	0.08
	(0.18)	(0.18)	(0.16)
Fraction Hispanic	0.11	0.12	0.13
	(0.18)	(0.19)	(0.20)
Fraction Free-Reduced Price Lunch	0.38	0.39	0.43
	(0.23)	(0.23)	(0.22)
Number of Students	5,265	5,330	4,493
	(24,308)	(22,954)	(24,533)
Median Household Income	56,495	55,852	57,034
	(21,955)	(21,470)	(22,895)
Property Wealth ($100,000s)	22,588	24,630	20,729
	(127,459)	(133,268)	(112,742)
Districts	2,825	2,824	5,857
N	23,082	18,061	27,160

Notes: Means are reported with standard deviations in parentheses. Statistics are calculated for three estimation samples. The 10th grade cohort graduation rates with 1 lag are available from 2000-2010 and with 4 lags are available from 2003-2010. SEDA test scores are available for 2009-2013 so the spending variables lagged 5 to 8 years cover years 2001-2008. All monetary variables are in real 2013 dollars.

Table 1.2: Summary statistics for characteristics of districts in and out of the sample

	(1) In Sample	(2) Not In Sample	(3) P-value of (2)-(1) or Fraction	(4) All
	I. District-level Averages			
Number of Students	3,703	3,754	0.848	3,725
Number of Teachers	255	220	0.042	239
Student-Teacher Ratio	13.5	14.8	0.000	14.1
Spending Per Student	15,169	14,719	0.481	14,970
Property Tax Revenue Per Student	5,742	4,925	0.002	5,381
Median Household Income	56,328	53,546	0.000	55,111
Fraction with an IEP	0.131	0.136	0.000	0.133
Fraction FRPL Eligible	0.405	0.407	0.632	0.406
Fraction Black	0.077	0.063	0.000	0.071
Fraction Hispanic	0.116	0.105	0.001	0.111
Fraction White	0.743	0.751	0.092	0.747
	II. Observation Counts			
Students	26,592,100	21,418,996	0.55	48,011,096
Districts	7,182	5,706	0.56	12,888
States	24	26	0.48	50

Notes: Panel I displays averages for the variables indicated and panel II displays counts. Columns (1), (2), and (4) report averages of the indicated variables. Column (3) reports the p-value of the difference between column (2) and column (1) for panel I and the fraction in the sample for panel II. Values are based on 2009.

Table 1.3: First stage estimates of log simulated revenue on log spending

	(1) 1 year	(2) 1-4 years	(3) 1-8 years	(4) 5-8 years
A. 8th Grade Graduation Cohort Sample				
Log Sim. Rev.	0.157**	0.175**	0.188**	0.041
	(0.029)	(0.027)	(0.030)	(0.047)
F	29.63	43.02	39.61	0.77
Districts	2,724	2,720	2,667	2,668
N	17,467	15,676	9,146	9,147
B. 9th Grade Graduation Cohort Sample				
Log Sim. Rev.	0.145**	0.188**	0.214**	0.106**
	(0.024)	(0.024)	(0.027)	(0.042)
F	35.82	60.26	65.31	6.36
Districts	2,825	2,825	2,815	2,816
N	22,130	18,641	10,009	10,011
C. 10th Grade Graduation Cohort Sample				
Log Sim. Rev.	0.147**	0.180**	0.206**	0.105**
	(0.022)	(0.025)	(0.027)	(0.042)
F	44.42	51.74	60.09	6.21
Districts	2,825	2,824	2,816	2,817
N	23,082	18,061	9,967	9,969
D. 11th Grade Graduation Cohort Sample				
Log Sim. Rev.	0.150**	0.179**	0.216**	0.118**
	(0.023)	(0.027)	(0.026)	(0.042)
F	44.62	42.54	66.31	7.82
Districts	2,823	2,815	2,797	2,798
N	20,606	15,355	9,538	9,540
E. SEDA Test Score Sample				
Log Sim. Rev.	0.057+	0.037	0.221**	0.213**
	(0.032)	(0.025)	(0.018)	(0.022)
F	3.19	2.29	144.38	92.76
Districts	5,649	5,648	5,644	5,650
N	24,116	24,102	24,087	24,114

Notes: This table reports the results of first stage regressions of total expenditures on simulated revenue averaged over various previous years. Column (1) is the current and previous year, column (2) is the current through past 4 years, column (3) is the past 8 years, and column (4) is from 5 to 8 years prior to the measured outcome. Models also include controls for property wealth, median household income, fraction black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: + $p < 0.1$ * $p < 0.05$, ** $p < 0.01$.

Table 1.4: Two-stage least squares estimates of log spending on graduation rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	8th Grade Cohort		9th Grade Cohort		10th Grade Cohort		11th Grade Cohort	
	1 year	1-4 years	1 year	1-4 years	1 year	1-4 years	1 year	1-4 years
Log Spending	0.437**	0.265**	0.331**	0.307**	0.246**	0.384**	0.211**	0.382**
	(0.118)	(0.102)	(0.094)	(0.097)	(0.069)	(0.098)	(0.066)	(0.114)
Dep. Var. Mean	0.79	0.79	0.76	0.76	0.81	0.81	0.85	0.85
First-stage F	71.92	101.64	86.32	113.12	97.83	101.85	96.56	92.90
Districts	2,676	2,660	2,823	2,817	2,824	2,821	2,819	2,802
N	17,419	15,616	22,128	18,633	23,081	18,058	20,602	15,342

Notes: This table reports results from two-stage least squares regressions of graduation rates on average lagged log total expenditures instrumented with average lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.5: Two-stage least squares estimates of log spending on test scores

	(1)	(2)	(3)	(4)
	4th Grade		8th Grade	
	Math	Reading	Math	Reading
Log Spending, 5-8 years prior	0.775*	0.879**	0.929*	0.477+
	(0.338)	(0.304)	(0.401)	(0.277)
First-stage F	183.01	184.92	98.73	180.67
Districts	5,662	5,659	5,640	5,671
N	26,371	26,380	24,582	26,457

Notes: This table reports results from two-stage least squares regressions of test scores on average lagged log total expenditures instrumented with average lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.6: Two-stage least squares estimates of log spending on student composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Graduate Rates					SEDA Test Scores				
	Total	Black	Hispanic	IEP	FRPL	Total	Black	Hispanic	IEP	FRPL
Log Spending	2323.970**	0.022*	0.128**	0.023**	-0.064	2095.343**	0.009	0.159**	0.001	0.007
	(424.656)	(0.010)	(0.015)	(0.008)	(0.040)	(621.043)	(0.008)	(0.019)	(0.014)	(0.040)
Dep. Var. Mean	5265.24	0.11	0.11	0.13	0.38	4613.16	0.08	0.12	0.14	0.42
First-stage F	195.99	195.99	195.99	195.99	195.99	124.47	124.47	124.47	124.47	124.47
Districts	2,824	2,824	2,824	2,824	2,824	5,524	5,524	5,524	5,524	5,524
N	23,081	23,081	23,081	23,081	23,081	23,988	23,988	23,988	23,988	23,988

Notes: This table reports results from two-stage least squares regressions of student composition outcomes on average lagged log total expenditures instrumented with average lagged log simulated revenue. The outcome for the first column is total number of students, while the outcome for columns (2) through (5) are the fraction of students in the given category. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.7: Two-stage least squares estimates of future log spending on graduation rates

	(1)
Average Log Spending, Next 4 Years	-0.003
	(0.059)
F	95.06
Districts	2,824
N	23,438

Notes: This table reports results from two-stage least squares regressions of graduation rates on average log total expenditures over the next four years instrumented with log simulated revenue averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.8: Two-stage least squares estimates of spending on source of revenue

	(1)	(2)	(3)	(4)	(5)	(6)
	Graduation Rate			SEDA Test Score		
	Local	State	Federal	Local	State	Federal
Spending ($1,000s PP)	0.707**	0.103**	0.005	1.314**	-0.119	-0.080**
	(0.068)	(0.042)	(0.011)	(0.253)	(0.083)	(0.023)
Dependent Variable Mean ($1,000s PP)	5.57	5.93	0.94	5.95	5.88	0.87
Baseline Fraction	0.42	0.50	0.09	0.45	0.47	0.09
F	83.60	83.60	83.60	21.42	21.42	21.42
Districts	2,824	2,824	2,824	5,527	5,527	5,527
N	23,081	23,081	23,081	24,006	24,006	24,006

Notes: This table reports results from two-stage least squares regressions of various sources of revenue on average lagged log total expenditures instrumented with log simulated revenue averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.9: Two-stage least squares estimates of spending on total expenditure sub-categories – graduation rate sample, 10th grade cohort, one-year lag

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Current	Non-Elementary	Capital	Payments to:				Interest
	Expenditure	or Secondary	Outlay	State	Other Schools	Private	Charter	Payments
Spending ($1,000s PP)	0.515**	0.045**	0.259**	0.122**	0.012*	0.016**	0.002	0.036**
	(0.051)	(0.009)	(0.061)	(0.028)	(0.006)	(0.005)	(0.006)	(0.009)
Dep. Var. Mean	10.67	0.07	1.24	0.06	0.12	0.064	0.021	0.24
Baseline Fraction	0.87	0.01	0.09	0.004	0.01	0.004	0.002	0.02
First-stage F	83.60	83.60	83.60	83.60	83.60	83.60	83.60	83.60
Districts	2,824	2,824	2,824	2,824	2,824	2,824	2,824	2,824
N	23,081	23,081	23,081	23,081	23,081	23,081	23,081	23,081

Notes: This table reports results from two-stage least squares regressions of various expenditure categories on average lagged log total expenditures instrumented with log simulated revenue averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.10: Two-stage least squares estimates of log spending on total expenditure sub-categories – SEDA sample, five to eight year lag

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Current	Non-Elementary	Capital	Payments to:				Interest
	Expenditure	or Secondary	Outlay	State	Other Schools	Private	Charter	Payments
Spending ($1,000s PP)	0.571**	-0.005	0.229+	0.083**	0.093**	0.020**	-0.011*	0.041**
	(0.118)	(0.005)	(0.121)	(0.024)	(0.028)	(0.008)	(0.005)	(0.013)
Dep. Var. Mean	10.75	0.06	1.26	0.05	0.24	0.067	0.018	0.24
Baseline Fraction	0.86	0.00	0.08	0.004	0.02	0.005	0.003	0.02
First-stage F	21.42	21.42	21.42	21.42	21.42	21.42	21.42	21.42
Districts	5,527	5,527	5,527	5,527	5,527	5,527	5,527	5,527
N	24,006	24,006	24,006	24,006	24,006	24,006	24,006	24,006

Notes: This table reports results from two-stage least squares regressions of various expenditure categories on average lagged log total expenditures instrumented with log simulated revenue averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.11: Two-stage least squares estimates of log spending on current expenditure sub-categories

	(1)	(2)	(3)	(4)	(5)	(6)
		Graduate Rates			SEDA Test Score	
	Instructional	Support Services	Other	Instructional	Support Services	Other
Spending ($1,000s PP)	0.292**	0.215**	0.007**	0.420**	0.159**	-0.008+
	(0.032)	(0.024)	(0.003	(0.086)	(0.042)	(0.004
Dep. Var. Mean	6.55	3.69	0.44	6.57	3.74	0.44
Baseline Fraction	0.53	0.30	0.04	0.52	0.30	0.04
First-stage F	83.60	83.60	83.60	21.42	21.42	21.42
Districts	2,824	2,824	2,824	5,527	5,527	5,527
N	23,081	23,081	23,081	24,006	24,006	24,006

Notes: This table reports results from two-stage least squares regressions of various current expenditure categories on average lagged log total expenditures instrumented with log simulated revenue averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table 1.12: Two-stage least squares estimates of log spending on graduation rates

A. Gains versus Losses

	(1) 8th Grade	(2) 9th Grade	(3) 10th Grade	(4) 11th Grade
Log Spending	0.413**	0.308**	0.225**	0.198**
	(0.110)	(0.088)	(0.065)	(0.063
Loss*Log Spending	-0.006	0.009	0.009	0.031**
	(0.013)	(0.011)	(0.010)	(0.011)
Dep. Var. Mean	0.79	0.76	0.81	0.85
Kleibergen-Paap F	38.67	44.33	48.64	45.76
Districts	2,676	2,823	2,824	2,819
N	17,419	22,128	23,081	20,602

B. High- versus Low-Income Districts

	(1) 8th Grade	(2) 9th Grade	(3) 10th Grade	(4) 11th Grade
Log Spending	0.706**	0.594**	0.431**	0.432**
	(0.255)	(0.215)	(0.148)	(0.145
Low Income*Log Spending	-0.561*	-0.557**	-0.370*	-0.401**
	(0.270)	(0.229)	(0.159)	(0.155)
Dep. Var. Mean	0.79	0.76	0.81	0.85
Kleibergen-Paap F	12.06	12.87	15.55	15.14
Districts	2,676	2,823	2,824	2,819
N	17,419	22,128	23,081	20,602

Notes: This table reports results from two-stage least squares regressions of graduation rates (panel A) and test scores (panel B) on average lagged log total expenditures instrumented with log simulated revenue, averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. All covariates are interacted with an indicator equal to 1 if the change in simulated revenue from the previous year is negative. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

CHAPTER 2

THE INTERGENERATIONAL IMPACT OF CIGARETTE TAXES ON SMOKING INITIATION

2.1 Introduction

Tobacco use is the leading preventable cause of death in the United States and generates enormous individual and social costs. Indeed, estimated external costs of cigarette smoking are over $300 billion per year.[1] Policymakers often use cigarette taxes to reduce smoking and its associated costs. Over the last four decades, real cigarette taxes have increased from an average of $0.51 per pack in 1976 to $1.53 in 2014 (Orzechowski and Walker, 2014). Although cigarette taxes are shown to reduce the total demand for cigarettes (Chaloupka and Warner, 2000), it is unclear whether they help convince young people to not start smoking. The act of preventing people from starting to smoke is likely more efficient than encouraging them to quit because smoking is addictive (U.S. Department of Health & Human Services, 2014). Nevertheless, in a recent review, Guindon (2014) concludes that empirical evidence is "too limited to make any conclusive statements about the impact of tobacco prices or taxes on smoking onset." This lack of consistent evidence has led many to argue that cigarette taxes are an ineffective policy tool for reducing smoking initiation.

Though the methods and results of previous studies vary, they all focus exclusively on the direct effect of cigarette taxes on individuals' own smoking behavior.

[1]This figure includes around $170 billion in direct medical costs (Xu et al., 2015), the majority of which are payed for by public programs, and over $156 billion in productivity lost to decreased health and premature death (U.S. Department of Health & Human Services, 2014).

There is little evidence that contemporaneous cigarette taxes reduce youth initiation in particular. This focus on the immediate impact may overlook an important indirect effect that flows through the smoking behavior of other individuals in the community. Older friends and family members may play an especially large role in a youth's decision to begin smoking. This paper is the first to identify the existence and magnitude of an intergenerational effect of cigarette taxes on smoking initiation.[2] Specifically, we link changes in cigarette taxes during early childhood to later initiation decisions. This linkage is formed by a two-stage process. First, many studies find a negative effect of cigarette taxes on adult smoking behavior (Chaloupka and Warner, 2000). Then, we propose that the smoking behavior of socially connected adults during an individual's childhood influences his or her decision to begin smoking as a youth.[3] Evidence relating smoking initiation to cigarette taxes in early childhood through the smoking behavior of older people in the community provides important new insight to the discussion of the efficacy of cigarette taxes.

We use data from the National Longitudinal Survey of Children and Young Adults (NLSCYA) to estimate a hazard model of smoking initiation that allows for an indirect, intergenerational effect of taxes. Cigarette taxes from childhood might also affect smoking through serial correlation with contemporaneous cigarette taxes and their direct effect on smoking initiation. Thus, we estimate the effect of tax changes in

[2]We use the term intergenerational in the broad sense of a connection between an older and younger person and not solely between parents and their offspring.

[3]There is a strong correlation between the smoking behavior of parents, friends, and other close connections and youth smoking initiation (Jackson and Henriksen, 1997; Avenevoli and Merikangas, 2003; Hill et al., 2005; Bricker et al., 2007; Gilman et al., 2009; Göhlmann, Schmidt, and Tauchmann, 2010). This relationship reflects an intergenerational correlation in smoking behavior to the extent that the two compared people are different ages. There is no clear evidence of whether this correlation is driven by a causal relationship.

early childhood (before smoking onset decisions) while also controlling for contemporaneous cigarette taxes, which isolates the relationship between cigarette taxes during childhood and later smoking initiation through an intergenerational channel. Our estimates show that a real cigarette tax increase of $0.25 during childhood decreases the hazard of later initiating smoking by 12.5 percent. This intergenerational effect is several times larger than the direct effect estimated in prior work.[4] A back-of-the-envelope calculation suggests that a current $0.25 increase in cigarette taxes will save $68 billion in lifetime external costs by reducing the number of children who later begin smoking. This gives perspective to the benefits of cigarette taxes missed by prior work by not considering this intergenerational channel.

This paper provides four contributions to the literature. We are the first to estimate the indirect effect of cigarette taxes on smoking initiation through an intergenerational channel. Our second contribution is evidence for the existence of an intergenerational social transmission of smoking behavior. This evidence is consistent with peer effects in smoking decisions, but uses a potentially more robust research design than regressing smoking initiation on peer smoking decisions which can suffer from reflection and endogeneity bias due to "common shocks" (Angrist, 2014). Third, we are the first to use the NLSCYA to replicate the finding that

[4]Most studies report a range of estimates based on several models and variable definitions. If we focus on the largest magnitude price/tax elasticity from each study that finds a negative effect (Douglas and Hariharan, 1994; Forster and Jones, 2001; Tauras, O'Malley, and Johnston, 2001; DeCicca, Kenkel, and Mathios, 2002, 2008; Glied, 2002; Cawley, Markowitz, and Tauras, 2004, 2006; Coppejans et al., 2007; Nonnemaker and Farrelly, 2011; Lillard, Molloy, and Sfekas, 2013), the average is -0.62 with the largest magnitude being -1.52. This includes the estimated elasticities for models without state fixed effects in papers that also estimate models with state fixed effects and find no effect. By comparison, the intergenerational tax elasticity of initiation implied by our results is -5.39.

contemporaneous taxes do not have a significant direct effect on smoking initiation. Our final contribution is a method that uses full information from multiple reports to measure the age of smoking initiation.[5] Our estimates are unchanged by the application of this method, possibly because the recall window is rather short in the NLSCYA data. Nevertheless, our approach is useful in contexts that combine panel data and retrospective information.

Our findings suggest that past studies understate the effect of cigarette taxes on smoking initiation. The intergenerational effect we document is several times larger than any other estimates of the direct effect of cigarette taxes on smoking initiation. While the measure of a policy's efficacy ultimately does not depend on the channel through which it operates, understanding exactly how policies affect behavior helps to craft more targeted interventions in the future. Furthermore, the existence of an intergenerational transmission of smoking behavior suggests that the effect of other tobacco control policies is similarly multiplied through their impact on the next generation.

In the next section, we relate our results to the current policy environment, review previous studies of cigarette taxes and smoking initiation, and discuss potential mechanisms for an intergenerational effect of cigarette taxes. We discuss our empirical strategy in Section 2.3. In Section 2.4, we discuss the data and our method for dealing with multiple retrospective reports. We present and discuss our results in

[5]Studies that rely solely on retrospective smoking information either take a single response from a cross-section (Douglas and Hariharan, 1994; Hammar and Martinsson, 2001; López Nicolás, 2002; Kidd and Hopkins, 2004; Peretti-Watel, 2005; Madden, 2007) or use the response from a single year of a panel (Forster and Jones, 2001; Boudarbat and Malhotra, 2009; Lillard, Molloy, and Sfekas, 2013). These approaches make an ad hoc decision about which response to use and throw out the remaining information and are more sensitive to mismeasurement in a given year.

Section 2.5 and provide concluding remarks in Section 2.6.

2.2 Background

2.2.1 Policy Environment

Over the past several decades, the magnitude of each change in cigarette taxes increased while youth smoking decreased. This pattern is reflected in Figure 2.1, which shows the average magnitude of cigarette tax increases and the smoking participation rate of 12th graders from 1976 to 2014.[6] The negative time-series relationship between tax increases and youth smoking participation is also confirmed by formal analyses (Carpenter and Cook, 2008; Hansen, Sabia, and Rees, 2017). Despite this evidence on youth smoking participation, the literature remains undecided on whether cigarette taxes deter smoking initiation.

Currently, state excise taxes on a pack of 20 cigarettes range from a low of \$0.17 in Missouri to a high of \$4.35 in New York (Orzechowski and Walker, 2014). Many states are also considering additional cigarette taxes. For example, four states voted on significant cigarette tax increases in 2016, the average size of which was \$1.59.[7]

[6]The rate of smoking participation among youth has been measured more consistently over time than initiation behavior, so we discuss the trends in youth smoking in terms of the participation rate.

[7]California Proposition 56 (approved): raised per pack tax from \$0.87 to \$2.87; Colorado Amendment 72 (defeated): would have raised per pack tax from \$0.84 to \$2.59; Missouri Proposition A and Constitutional Amendment 3 (defeated): would have raised per pack tax from \$0.17 to \$1.00 (combined); and North Dakota Measure 4 (defeated): would have increased per pack tax from \$0.44 to \$2.20.

2.2.2 Lessons from Literature on Cigarette Taxes and Smoking Initiation

Empirical evidence on the effect of cigarette taxes on smoking initiation is inconsistent. For example, some studies find no effect of cigarette taxes on smoking initiation (Douglas and Hariharan, 1994; DeCicca, Kenkel, and Mathios, 2002; Madden, 2007; DeCicca, Kenkel, and Mathios, 2008), while others find that cigarette taxes are negatively related to initiation in Spain (López Nicolás, 2002) and among certain demographic subgroups in the United States (Cawley, Markowitz, and Tauras, 2004; Nonnemaker and Farrelly, 2011). Guindon (2014) reviews 27 papers, most with important limitations, and concludes that the current literature is insufficient to form a consensus about whether or not cigarette taxes or prices affect smoking initiation. One potential reason for the inconsistency of findings is the inconsistency of methods used between studies.

Many of these studies do not include state fixed effects. This omission is often due to a lack of sufficient within-state variation in cigarette taxes. Those studies that are unable to include state fixed effects still discuss the importance of controlling for unobserved state characteristics and attempt to do so with other observed state characteristics (e.g. indicators for tobacco producing states in Cawley, Markowitz, and Tauras, 2004). However, the inclusion or exclusion of state fixed effects does not fully explain the inconsistent findings in the literature. For instance, DeCicca, Kenkel, and Mathios (2002, 2008) find a negative effect of cigarette taxes on smoking initiation in specifications without state fixed effects, but when fixed effects are included the coefficient on taxes is slightly positive and not statistically significant. Also, Lillard, Molloy, and Sfekas (2013) include state fixed effects in all their models but find a negative effect that is not statistically significant in all specifications. We find a small, negative effect of contemporaneous cigarette taxes on initiation that is not robust to the inclusion of state fixed effects, which is consistent with DeCicca, Kenkel, and Mathios (2002, 2008). By comparison, the indirect effect of taxes from childhood is robust to the inclusion of state fixed effects or state time trends.

Many studies are also unable to accurately determine exposure to cigarette taxes over time due to limited geographic information. For example, many studies use state of birth or state at the time of data collection to calculate the taxes an individual faces at the time of initiation. This method assumes the individual has not moved states either between birth and initiation or between the time of initiation and data collection. We require fewer assumptions about mobility because the NLSCYA gives the actual state of residence for most years since birth.[8]

Previous studies also differ in the way they measure initiation. Most studies either compare smoking status between waves in a longitudinal dataset or use retrospective reports on the age of smoking initiation. The approach using the change in smoking status between waves neglects individuals who begin smoking prior to entering the survey[9] and suffers from measurement error when follow ups are infrequent. The

[8] In alternative analyses, we find that our results hold even if we assume people remain in their state of birth or were born in the state they lived when we first observe their smoking behavior. This suggests the intergenerational effect we detect would also be observed using other, more limited data sources.

[9] One exception is Nonnemaker and Farrelly (2011), who supplement their longitudinal data with retrospective information to measure the timing of initiation before respondents are observed in the data.

two primary drawbacks of the retrospective approach are misreported age of initiation and lack of demographic characteristics from the time of the initiation decision. Measurement error in the age of smoking initiation becomes more of a concern as recall bias increases due to asking individuals about events farther in the past. In addition, later-in-life demographic characteristics, such as eventual educational attainment and family income, are endogenous to earlier smoking behavior (Kenkel, Lillard, and Mathios, 2006). We use the retrospective method, but are able to reduce the impact of these common problems. Individuals in our data are asked the age they started smoking beginning at ten years old, so recall bias is minimized. Also, because our data originally focused on the parents of our respondents, we are able to control for family demographic information contemporaneous to the initiation decision no matter how young respondents started smoking.

2.2.3 Potential Mechanisms of Smoking Transmission

Correlational studies find that youth are more likely to begin smoking if their parents, friends, or other close connections smoke (Jackson and Henriksen, 1997; Avenevoli and Merikangas, 2003; Hill et al., 2005; Bricker et al., 2007; Gilman et al., 2009; Göhlmann, Schmidt, and Tauchmann, 2010). Although these correlations may be driven by unobserved factors, there is reason to believe that relationships with smokers have a causal impact on smoking initiation. Potential causal mechanisms include social pathways such as role-model effects, transmission of social norms, or increased access to cigarettes, and physiological pathways such as nicotine addiction from secondhand smoke or in-utero exposure.

We directly test whether the documented correlations are causal by estimating the effect of cigarette taxes from childhood on smoking initiation. If the intergenerational correlation does not reflect any causal link, then ceteris paribus changes in cigarette taxes faced exclusively by an older cohort should have no effect on the smoking habits of the younger cohort. A significant effect of cigarette taxes during childhood, then, is evidence for a causal mechanism connecting smoking habits between generations.

Although our estimation strategy does not require that we specify a particular causal mechanism, the policy implications of our results depend on whether the intergenerational effect occurs exclusively via parents or includes other adults in the community as well. Children interact socially with many adults, but parents may be the adults they interact with the most. Thus, while we expect that adults other than parents influence a youth's smoking initiation (e.g. other adults can be role models or provide access to cigarettes), the transmission of smoking behavior may be more concentrated within families. Cigarette taxes are unlikely to affect non-smokers, so if taxes from childhood affect smoking initiation even when parents have never smoked then we take this as evidence that other non-parent adults influence youth initiation. Additionally, a mother's behavior while pregnant has a unique effect on her children. Simon (2016) finds that increased cigarette taxes cause pregnant mothers to smoke less, which improves the health of their children. We can isolate the effect of these changes to health by considering the effect of taxes while in utero separately from the rest of childhood. If smoking initiation is driven by the physiological effects of in-utero exposure as opposed to the social influence of changes in the behavior of parents or other adults, we would expect the effect to be concentrated on taxes while in utero. Our analyses suggest that the intergenerational transmission of smoking behavior is not fully explained by parent-child interactions.

2.3 Empirical Strategy

Our primary strategy estimates the effect of cigarette taxes from birth to age seven on later smoking initiation.[10] We control for other factors that may influence smoking initiation, including contemporaneous cigarette taxes. Specifically, we estimate a discrete-time hazard model of the following form:

$$\lambda_{ist}(\text{age}) = \lambda_0(\text{age})g\left(\beta_1\text{Tax}_i^{0-7} + \beta_2\text{Tax}_{st} + \mathbf{X}_{it}\cdot\boldsymbol{\alpha} + \gamma_s + \gamma_t\right) \tag{2.1}$$

where λ_{ist} is the hazard of smoking initiation as a function of age for person i living in state s in year t, $\lambda_0(\text{age})$ is the baseline hazard (i.e. the hazard at each age given average characteristics), $g(\cdot)$ is the inverse complementary log-log function ($y = 1 - \exp(-\exp(x))$),[11] Tax_i^{0-7} is the time-invariant mean cigarette tax from birth to age seven for individual i,[12] Tax_{st} is the state-level cigarette tax corresponding to the current state of residence, and $\mathbf{X}_{it}$ is a vector of controls for sex, race/ethnicity, family income, mother's age when individual i was born, birth order, parent smoking history,

[10]Our main strategy assumes individuals are not at risk of smoking until age eight but results are robust when we allow the hazard to begin at other ages. Estimates with start ages ranging from six to ten are available in Appendix Table B.1.

[11]We explore various functional forms for $g(\cdot)$ in Appendix Table B.2. The results are larger in magnitude with logit or probit specifications, so the complementary log-log specification provides a conservative estimate as well as having the useful property that exponentiated coefficients return hazard ratios.

[12]When we assume the age an individual is first at risk of initiation is something other than 8, this variable is defined as the average cigarette tax from birth until one year before individuals are assumed to be at risk.

and parents' highest educational attainment. The baseline hazard is estimated by individual indicators for each age with the constant term omitted. We also include γ_s and γ_t, which are state and year fixed effects, respectively. We account for within-state correlation in factors that influence smoking initiation by clustering standard errors at the state level.

State fixed effects account for time-invariant smoking behaviors and/or attitudes at the state-level (such as being a tobacco producing-state or a state with high levels of anti-smoking sentiment). Year fixed effects account for differences in the underlying smoking initiation hazard across cohorts.[13] Demographic controls are motivated by previous research, which suggests race, sex, parent characteristics, and socioeconomic status (including income and education) are important determinants of smoking behavior (e.g. Chaloupka and Pacula, 1999; Powell and Chaloupka, 2005; Nonnemaker and Farrelly, 2011).

The main coefficient of interest is β_1, which is the effect of cigarette taxes during early childhood on the hazard of initiation. This coefficient is identified by within-state changes in cigarette taxes over time and differences across states in the size and timing of tax increases. Policy endogeneity is a primary concern in this type of estimation strategy. Nevertheless, the assumption that changes to state cigarette taxes are exogenous to individual smoking decisions is ubiquitous in the literature. Legislatures increasing cigarette taxes in response to either increases in youth smoking rates (as seen in the 1990s) or rising anti-smoking sentiment threatens a causal interpretation of β_2, or the coefficient on the contemporaneous tax. However, one

[13]Due to the age fixed effects, the year fixed effect is equivalent to including birth year fixed effects.

strength of considering lagged cigarette taxes is that legislators are unlikely to base policy on future trends in smoking rates of infants or young children. Another potential threat to identification is if movement between states is related to unobserved smoking preferences. To address this concern, we estimate Equation 2.1 separately for those who ever moved states and those who never moved and we find similar results for both groups.[14] Further, our results are similar if we measure the average tax during childhood using a fixed measure for state of residence in all years (e.g. state of birth or state of residence at age 8) regardless of actual interstate mobility, or simply the tax in the state and year of birth.[15]

Our main effect is slightly larger in magnitude when we estimate a version of Equation 2.1 with state-specific linear time trends.[16] However, isolating pre-treatment trends for many overlapping tax increases is problematic, so we are unable to evaluate the expected effect of these trends on our estimates (see Wolfers, 2006). Our preferred specification does not include these trends, which provides a more conservative estimate that also uses fewer degrees of freedom.

Equation 2.1 is our preferred specification, but we present results for various subsamples and additional specifications. To distinguish between familial and other social influences, we estimate Equation 2.1 separately for subsamples split by parent smoking history. We also explore the importance of in-utero exposure by adding a measure of cigarette taxes in the year before birth to the model. This helps

[14]The difference in the intergenerational effect between these two subsamples is not statistically significant ($p = 0.609$) in a fully interacted model. The results for separate subsamples are available in columns (2) and (3) of Appendix Table B.3 and the results of the fully interacted model are available upon request.

[15]These results are available in columns (2) through (4) of Appendix Table B.4.

[16]These results are available in column (1) of Appendix Table B.4.

distinguish between the physiological effects of mother's smoking while pregnant, such as those seen in Simon (2016), and social transmissions of smoking behavior.

2.4 Data

2.4.1 NLSCYA

The National Longitudinal Study of Youth 1979 (NLSY79) is a nationally representative sample of 12,686 young men and women who were 14-22 years old when they were first surveyed in 1979. These individuals were interviewed annually through 1994 and are currently interviewed on a biennial basis by the Bureau of Labor Statistics. In 1986, the NLSY began surveying the biological children of female participants biennially from birth to age 14 in the Child (C) survey. Beginning in 1994, the NLSY also includes a Young Adult (YA) survey for these children who are age 15 and older. We use information from all three of these surveys, which we refer to as the National Longitudinal Survey of Children and Young Adults (NLSCYA). This sampling pattern results in a sample that is not nationally representative of children in these cohorts, rather the sample is representative of children born to mothers in the original NLSY79 sampling frame.

Our outcome of interest is smoking initiation. Participants answer questions about their tobacco use from age 10 to 14 in the C survey and for all ages in the YA survey. While some questions differ between the surveys, all individuals in our sample gave the age at which they first smoked cigarettes.[17] We describe how we

[17] The exact wording in the C sample is "How old were you when you first smoked a cigarette?"

use the answers to these questions to create our measure of smoking initiation in Section 2.4.2. Information about parent smoking behavior comes from the NLSY79 and is only available for mothers. We therefore measure parent smoking history as whether an individual's mother has smoked at least 100 cigarettes in her life. The demographic controls in our model include sex, race, family income, and the highest educational attainment of either parent.[18] We include additional controls to account for the selection of birth-year cohorts into our sample. Because our sample is made up of children from a fixed cohort of mothers, those born in earlier years are born to a mother that is younger and generally more disadvantaged. To account for this, we include measures for mother's age at the focal child's birth and birth order.

Our independent variable of interest is the cigarette excise tax levels to which children are exposed. In the restricted-access version of the NLSCYA, we observe the state of residence for each child since birth.[19] This lets us match cigarette tax information from the Tax Burden on Tobacco Historical Compilation (Orzechowski and Walker, 2014) to our sample. Taxes are adjusted for inflation and reported in 2014 dollars.

Our data is formatted as person-age observations. Therefore, each person contributes one observation for each age beginning at age 8 until age 25 or until that

and the wording is "How old were you the first time you smoked cigarettes" in the YA sample.

[18]The race categories given in the NLSCYA are Hispanic, Non-Hispanic black, and Non-black/Non-Hispanic. We categorize total family income by quartile. Specifically, we create a categorical variable for the quartile family income falls into for each year, then assign each individual to the average quartile their family's income falls into for the majority of their life. Parent education is categorized as less than high school diploma, high school diploma, some college, and BA or higher.

[19]This is available every year from 1979 to 1994, then biennially thereafter. We carry forward the most recent state of residence to fill in these gaps.

person reports having initiated smoking, after which they are dropped from the sample. Only 2 percent of our sample initiates before age 8 and we only observe 11 initiations past the age of 25, which amounts to 0.12 percent of our sample.[20] This pattern is consistent with a 2014 report of the U.S. Surgeon General indicating that 99 percent of smokers begin smoking before age 26 (U.S. Department of Health & Human Services, 2014). We use survey weights provided with the NLSCYA to adjust for the probability of mothers being sampled in the original 1979 survey in all summary statistics and analyses. These weights primarily adjust for the original oversampling of minority groups, and results are robust to whether or not we include these weights.[21]

Although we observe many individuals in each state, initiation is a relatively rare event. It is therefore not surprising that we do not observe anyone initiate smoking in some less-populous states. If we do not observe an initiation for a state, then the predicted probability of initiating is 0 within that state, which in turn causes the fixed effect for that state to be estimated as $-\infty$. In practice, both the fixed effect and any observations from these states are dropped from the analysis. This problem is exacerbated when we stratify our sample and the number of states with no initiations increases. To balance our panel and allow comparisons between subsamples, we remove individuals from our data who ever report living in a state for which we do not observe an initiation event. These restrictions do not change the results enough to alter our conclusions, but the coefficients are smaller in magnitude

[20]Our results are not changed if we do not impose a right censor (available upon request).

[21]Unweighted summary statistics are available in Appendix Table B.5, and results are provided in Appendix Table B.6 and Appendix Table B.7.

after the restrictions. We therefore view our reported results as conservative in this regard. Ultimately, we restrict our sample to individuals with no missing information, who are born after 1976, who enter the sample prior to age 8, and never report living in Delaware, Hawaii, Idaho, Utah, Vermont, or Wyoming. Most of these restrictions amount to trimming out low-density areas of our data.[22] Our resulting estimation sample consists of 8,228 individuals from 45 states.

2.4.2 Measuring Smoking Initiation

Due to the longitudinal nature of the data, respondents report their age of smoking initiation up to six separate times. Approximately 12 percent of respondents (972 out of 8,228 individuals in the estimation sample) have a discrepancy in reported age of initiation. This could be due to measurement error at the time of data collection, misremembering the true age, intentionally providing an inaccurate report due to social desirability bias, or misunderstanding the intention of the question. However, for most people (91 percent), all reported ages are within one year so this does not constitute significant measurement error.

Most smoking initiation studies that rely solely on retrospective smoking information take a single response from a cross section (Douglas and Hariharan, 1994; Hammar and Martinsson, 2001; López Nicolás, 2002; Kidd and Hopkins, 2004; Peretti-Watel, 2005; Madden, 2007) or a single year of a panel (Forster and Jones, 2001;

[22] Only 2,680 of the 11,506 children and young adults in the NLSCYA have valid tobacco-use information. By comparison, only 103 (less than 1 percent) are removed because of our state limitations. Appendix Table B.8 reports summary statistics for the group included in our estimation sample compared to those omitted.

Boudarbat and Malhotra, 2009; Lillard, Molloy, and Sfekas, 2013). Strategies include using the first response recorded, the last response recorded, the modal response, the minimum response, or the maximum response. These approaches make an ad hoc decision about which response to use and throw out the remaining information. Using the modal response is attractive, but unless there is a single mode one must still make a decision about which mode to choose. Without additional information about the underlying reason for conflicting reports, each of these methods discards some of the information provided.

To retain as much information as possible, we average the smoking initiation age across multiple reports. Specifically, we code the smoking status at each age as the fraction of times an individual reports having started smoking by that age. This creates a variable that ranges from 0 to 1, with the possibility of values in between.[23] An example illustrates this procedure. Assume a respondent answers the smoking questions in 5 different waves. In two waves, she reports having initiated smoking at age 15, in one wave she reports 16, and in two waves she reports 17. The initiation status variable is then 0 for ages 14 and younger, 0.4 for age 15, 0.6 for 16, and 1 for ages 17 and older. We report results using the value of 0.5 as the cutoff to define age of smoking initiation, therefore our example respondent is coded as initiating at age 16. Results are robust to the choice of cutoff between 0 and 1 and to removing all individuals with a discrepancy from the analysis.[24] The robustness of our results in this regard is potentially due to the short retrospective window in the NLSCYA

[23]We remove responses if the reported starting age is larger than the person's age at the time the question was asked. We also remove responses of zero, as these appear to be a reporting error in the data.

[24]These additional analyses are available in Appendix Figure B.1 and Appendix Table B.9.

(children are asked about their smoking behavior as early as age 10).

2.4.3 Summary Statistics and Baseline Hazards

The distribution of birth-year cohorts and sample years are shown in Figure 2.2. Our sample includes individuals born between 1976 and 2004 who were at risk of smoking initiation between 1984 and 2014. Figure 2.2 also plots the number of states with tax increases for each year. This highlights the variation in cigarette tax faced both in childhood (all changes in the seven years following birth) and in adolescence, which identifies our coefficients of interest.

Our outcome of interest is the hazard of smoking initiation, which is the probability that an individual smokes at age a conditional on not having smoked prior to age a. We estimate the baseline hazard rate at each age by the number of initiations at the age divided by the number of individuals remaining in the sample. The fraction initiated is calculated as the fraction of individuals originally observed at age 8 who have initiated by a given age. The baseline hazard of smoking initiation and fraction initiated for each age in the estimation sample are graphed in Figure 2.3 Panels A and B, respectively. The average smoking initiation hazard across all ages is 0.054. The hazard gradually increases from about zero at age 8 to a peak of 0.14 at age 18 and then decreases rapidly. This pattern is also apparent in the fraction initiated in panel B, in which the curve increases steeply after age 10 and then quickly flattens out after age 20. Table 2.1 catalogues the calculation of the hazard rate at each age and shows the prevalence of right censoring in our estimation sample. The number of initiations outweighs the number censored (i.e. left the sample without initiating) until age 20, when censoring becomes more prevalent.[25] Of the original 8,228 individuals in our estimation sample, 1,691 are still in the sample and did not initiate by age 25.

Summary statistics for the rest of the variables used in our estimation sample are reported in column (1) of Table 2.2. Panel A reports time-invariant measures and Panel B reports measures that vary over time. Our sample includes 8,228 individuals which amounts to 89,289 individual-age level observations. People are removed from the sample after they initiate, so the individual-age level statistics are weighted toward people who never start smoking or who initiate at older ages. About 58 percent of people initiate within the sample, with the other 42 percent leaving the sample without initiating. The average cigarette excise tax faced from birth to age seven is \$0.45 and \$0.87 from age eight onward. The sample is evenly split between male and female respondents.

The summary statistics for the sample split by mother's smoking history are reported in columns (2) and (3) of Table 2.2. The proportion of the sample with a parent who ever smoked cigarettes is 59 percent. Mothers who smoke are on average less educated and make less money, and individuals whose mother ever smoked are 50 percent more likely to initiate smoking than those whose parents never smoked (6.5 percent vs. 4.1 percent).

Before turning to the results of our parametric models, we provide graphical evidence and non-parametric tests of the effect of cigarette taxes in childhood on

[25] To determine the importance of censoring for our results, we estimate our model on the sample of those observed until at least age 25 in Appendix Table B.10. The percentage point effect is the same as our main results.

smoking initiation. Figure 2.4 presents the graphs of the hazard function and the fraction initiated for those with a cigarette tax during childhood above and below the mean. 64.1 percent of those with an above-average cigarette tax in childhood ever initiate smoking compared to 52.9 of those who experienced a below-average tax. A non-parametric Wilcoxon test rejects the null hypothesis that the functions are the same with $p < 0.001$. This difference is driven by the lower hazard of initiation up to age 18, after which there is no difference in the hazard.

2.5 Results

We begin the discussion of our results with a note on interpreting coefficients from a hazard model. Exponentiated coefficients from a complementary log-log regression are interpreted as hazard ratios. For example, the exponentiated coefficient on the cigarette tax during childhood (e^{β_1}) represents how many times more likely someone with a one dollar higher tax during childhood is to initiate at any given age relative to someone who faces an average cigarette tax level. Values between 0 and 1 suggest a negative relationship between the variable of interest and the probability of smoking initiation; values greater than 1 suggest a positive relationship. Thus, statistical significance for hazard ratios is measured against the null hypothesis that the coefficient is equal to 1. Subtracting 1 from the hazard ratio gives the marginal effect of a $1.00 tax increase on the initiation probability. However, as we discuss in Section 2.2.1, the average tax increase in our study period is only $0.30, significantly less than the $1.00 increase implied by the hazard ratio. To interpret our estimates in a way that reflects the relevant policy variation, we report marginal effects of cigarette taxes relative to a \$0.25 increase in addition to the standard hazard ratios.[26] Mathematically, the reported marginal effect is given by:

$$\text{Marginal Effect} = (\exp(\beta) - 1) \times 0.25,$$

where β is the coefficient on the variable of interest.

We display the main results of our preferred specification in column (1) of Table 2.3.[27] Panel A shows standard hazard ratios and panel B reports the marginal effect of a \$0.25 increase. The hazard ratio for cigarette taxes during childhood is 0.498 with a corresponding marginal effect of -0.125 ($(0.498 - 1) \times 0.25 \approx -0.125$). Thus, a \$0.25 increase in the average cigarette tax during childhood reduces the hazard of later initiation by 12.5 percent (0.68 percentage points off a base of 5.4 percent). This estimate is statistically significant at the 5 percent level. The coefficient on the current cigarette tax is small, positive, and not statistically significant. Panel B of Figure 2.5 shows the baseline hazard of smoking initiation as well as the hazard given a \$0.25 increase in cigarette taxes during childhood. Our model includes a proportional hazards assumption,[28] so the percent effect is the same across ages, but the percentage point effect is largest at ages people are most likely to begin smoking (evidenced by a larger gap between the baseline and treated hazard graphs). The average effect of a \$0.25 increase in cigarette taxes across all ages is -0.7 percentage points ($-0.125 \times 0.054 \approx -0.007$). Similarly, panel A of Figure 2.5 shows that

[26] We choose \$0.25 as a benchmark because it lies between the average tax increase (\$0.30) and the standard deviation of taxes during the childhood of our respondents (\$0.21).

[27] The results for the full set of demographic controls is available in Appendix Table B.11.

[28] We find no evidence to contradict the validity of this assumption in alternative specifications where it is relaxed. These results are available upon request.

increasing cigarette taxes during childhood by \$1.00 decreases the hazard of smoking initiation by 50.2 percent (2.7 percentage points off a base of 5.4 percent).

Columns (2) and (3) of Table 2.3 report results for those whose mother ever smoked at least 100 cigarettes and for those whose mothers did not, respectively. The hazard ratio for cigarette taxes from childhood is 0.449 for those with a parent who smoked cigarettes and is 0.566 for those with never-smoking parents. Though the difference between these two estimates is not statistically significant ($p = 0.398$ in a fully interacted model), a closer evaluation of the differences between the two groups is informative. The marginal effects reveal the importance of considering the baseline hazard when interpreting the effect size. Those with a parent who ever smoked have a higher hazard of starting to smoke, on average (6.5 percent compared to 4.1 percent). Thus, the percentage-point effect of a \$0.25 increase in cigarette taxes during childhood is twice as large for those with a smoking parent than for those without (-0.897 percentage points compared to -0.447 percentage points), while the percent change is roughly the same for both groups (-13.8 percent for those with a smoking parent and -10.9 percent for those without). The difference in the percent and percentage point effects is further evident in Figure 2.6, which shows the baseline hazards and the hazards with a \$0.25 higher cigarette tax in childhood for each group. Those whose mother ever smoked have a higher baseline hazard (predominantly in the teenage years) and experience a larger intergenerational tax effect as evidenced by both the distance between baseline and treated hazard rates and the distance between average hazard lines. One implication of these results are that those with a higher baseline risk of initiation are potentially more responsive to policy intervention. Also, the fact that the intergenerational effect for those whose mother never smoked is nearly as large suggests that the causal link between generations is not solely driven by a familial mechanism.

Table 2.4 reports the effect of the contemporaneous cigarette tax separately from the effect of cigarette taxes in childhood. Controlling only for age and year effects, a \$0.25 higher cigarette tax is associated with a 0.9 percent lower initiation hazard. The magnitude of this relationship increases to -1.6 when we include demographic controls, but the effect disappears entirely with the inclusion of state fixed effects. This replicates the previous finding of a modest negative effect of cigarette taxes, which is not robust to the inclusion of state fixed effects (DeCicca, Kenkel, and Mathios, 2002, 2008). This supports the argument that the current price of cigarettes plays, at most, a small role in youth smoking initiation. By comparison, the final column shows that a \$0.25 increase in cigarette taxes from childhood decreases the initiation hazard by 12 percent. We also note that the standard errors on the tax variables only increase slightly when both are estimated simultaneously as in Table 2.3. This suggests that the estimates and their statistical significance are not driven by collinearity between the two measures of cigarette taxes.

Finally, the results for the model that separately considers cigarette taxes while in utero and taxes during childhood are reported in Table 2.5. The tax effects are not statistically significant in this specification. The hazard ratios for taxes in utero and taxes in childhood are similar at 0.692 and 0.788, respectively. In-utero exposure may play a role, but does not appear to be the dominant factor in intergenerational transmission of smoking behavior. This provides further support that the intergenerational effect constitutes a broader social phenomenon than just the biological connection between parents and children.

2.6 Conclusions

This paper poses a novel question: what is the indirect effect of cigarette taxes on youth smoking initiation via changes in the smoking behavior of older generations? We answer this by estimating the effect of cigarette taxes from before a person is at risk of initiating on their later smoking initiation. We find that a $0.25 increase in the average cigarette tax during childhood decreases the risk of initiating smoking at any given age by 12.5 percent (0.68 percentage points off of a base of 5.4 percent). This effect is robust to multiple specifications and independent of any direct effect of contemporaneous taxes. Youth are less likely to initiate smoking when older cohorts are exposed to higher cigarette taxes regardless of parent smoking history or whether the taxes are in utero versus early childhood. This provides evidence that the whole social environment, and not just families, is an important factor in the decision to begin smoking.

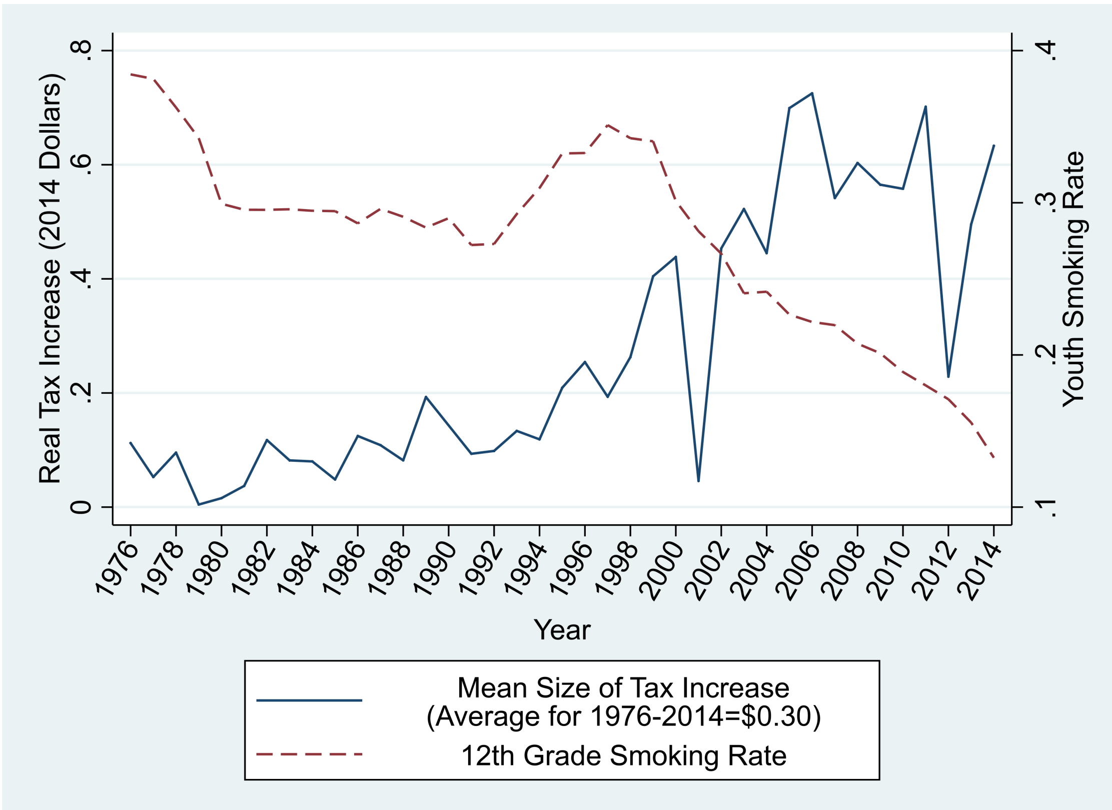
Figure 2.7 shows that a $0.25 cigarette tax during childhood decreases the fraction of people who ever initiate by 7.4 percentage points (0.588-0.514=0.074). A back of the envelope calculation indicates that a current federal tax increase of $0.25 will deter 2,380,592 children who are currently under 8-years-old from smoking in the future. Given that the average lifetime external cost of smoking is $28,500 (Sloan et al., 2004) this amounts to about $68 billion in savings. This rough calculation provides perspective for the magnitude of what is missed in previous research focused only on the direct effect of cigarette taxes. Other tobacco control policies may have similar intergenerational effects, so current estimates of their effects would also be understated.

The intergenerational effect we document provides additional evidence that neighbors can amplify the direct effect of policies on individual behavior. Other studies find this "social multiplier" effect in areas such as program participation and labor force decisions (Glaeser, Sacerdote, and Scheinkman, 2003; Duflo and Saez, 2003; Maurin and Moschion, 2009). In our context, there is no direct effect of taxes in childhood and what we measure is essentially the indirect social effect via older individuals.

Our ability to distinguish the causal mechanisms at play is limited by the data available. More comprehensive information about smoking attitudes and behaviors within families and communities is needed to disentangle the effect of different types of relationships. Once the most important mechanisms are identified, the structural relationship between cigarette taxes early in life and later smoking initiation can be estimated to gauge the magnitude of transmission from each relationship. Another data limitation is that our sample is based on a fixed cohort of mothers. This sample selection means that the birth year of our respondents is directly related to their mother's age at birth. Thus, we cannot disentangle differences in the intergenerational effect over time and differences between those whose mothers had children at different ages. Other data with a more representative distribution of parent characteristics across time will be able to overcome this limitation.

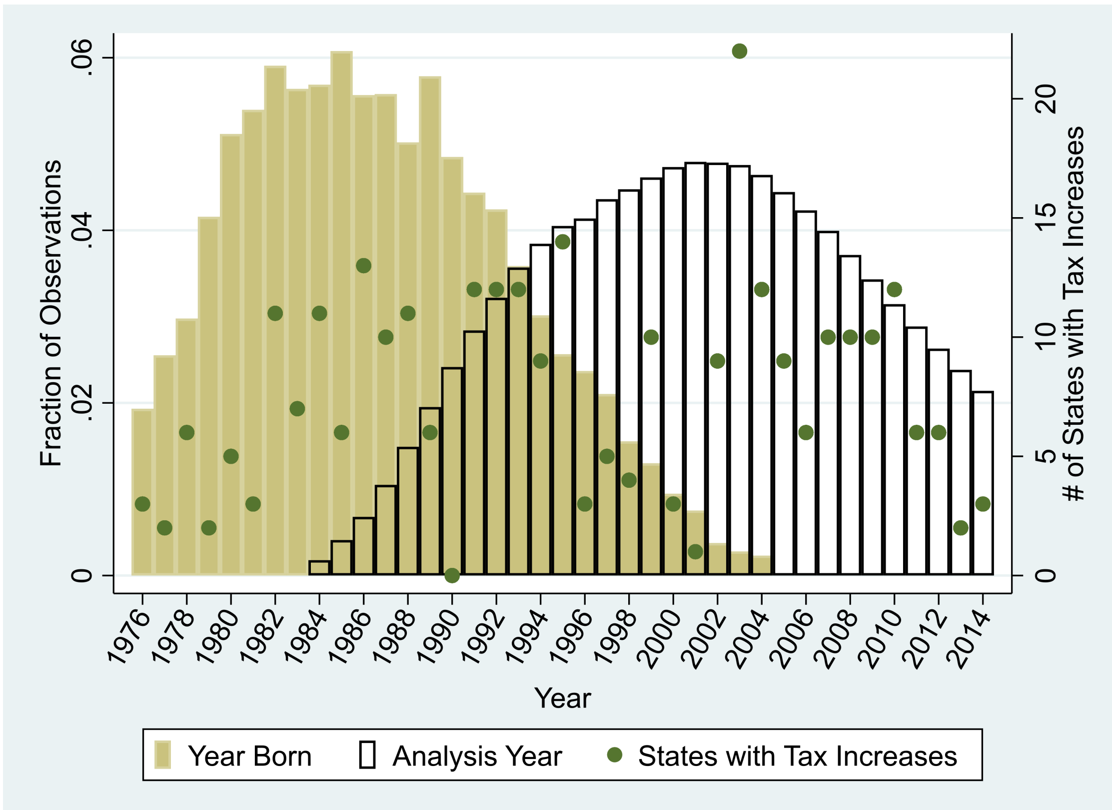
2.7 Figures & Tables

Figure 2.1: Cigarette Tax Increases and the 12th Grade Smoking Rate



Notes: The mean tax increase is based on data from the Tax Burden on Tobacco by Orzechowski and Walker (2014) and represents the average across all states that had any non-zero tax change in a given year. A value of zero corresponds to no cigarette tax increase in any state in that year. The 12th grade smoking rates are based on the Monitoring the Future national survey results on drug use (Johnston et al., 2017).

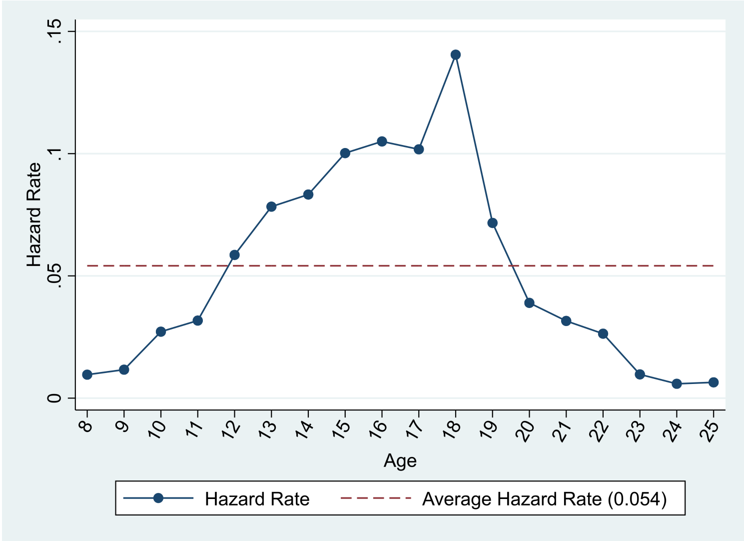
Figure 2.2: Sample Years and Timing of Cigarette Tax Increases



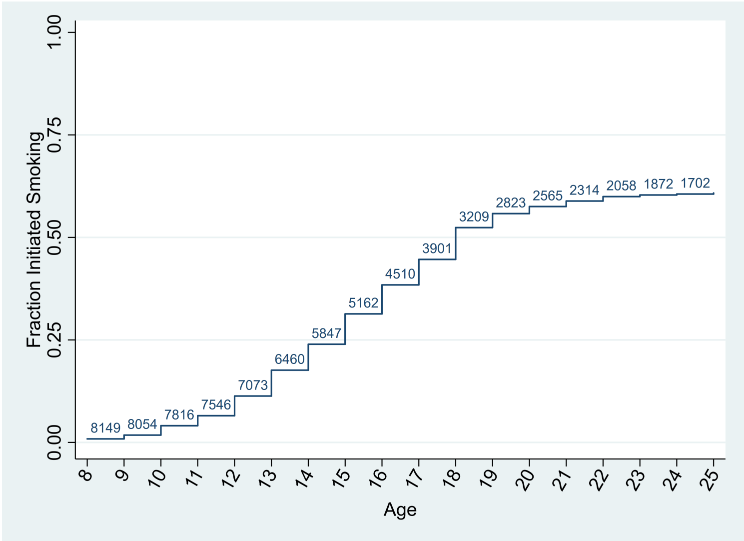
Notes: The y-axis on the left refers to the histograms of year born and year of analysis. The y-axis on the right corresponds to the scatter plot of states with tax increases in that year.

Figure 2.3: Baseline Smoking Initiation Hazard

A. Hazard Rate



B. Fraction Initiated Smoking



Notes: The hazard rate is the number of initiations at the given age divided by the number of individuals remaining in the sample. Panel B shows the fraction of of individuals we originally observe at age 8 who have initiated by a given age. The numbers on the graph are the number of individuals still at risk of initiating. The fraction of individuals that initiate by age 25 is 58 percent.

Table 2.1: Description of Smoking Initiation Hazard

Age	(1) # At Risk	(2) # Failed	(3) # Censored	(4) Hazard Rate	(5) Failure Function
8	8,228	79	0	0.010	0.010
9	8,149	95	0	0.012	0.021
10	8,054	219	0	0.027	0.048
11	7,816	248	19	0.032	0.078
12	7,546	442	22	0.059	0.132
13	7,073	554	31	0.078	0.199
14	6,460	538	59	0.083	0.264
15	5,847	586	75	0.100	0.336
16	5,162	542	99	0.105	0.401
17	4,510	459	110	0.102	0.457
18	3,901	548	150	0.140	0.524
19	3,209	230	144	0.072	0.552
20	2,823	110	156	0.039	0.565
21	2,565	81	148	0.032	0.575
22	2,314	61	170	0.026	0.582
23	2,058	20	195	0.010	0.585
24	1,872	11	166	0.006	0.586
25	1,702	11	1,691	0.006	0.588

Notes: Column (1) represents the number of individuals who are still in the sample and did not initiate by the given age. Column (2) reports the number of individuals who began smoking at the given age. Column (3) shows the number of individuals who left the sample before initiating. Column (4) is the hazard rate, which is calculated by dividing column (2) by column (1). Column (5) is the the fraction initiated, or the running sum of initiations divided by the original number at risk (8,228).

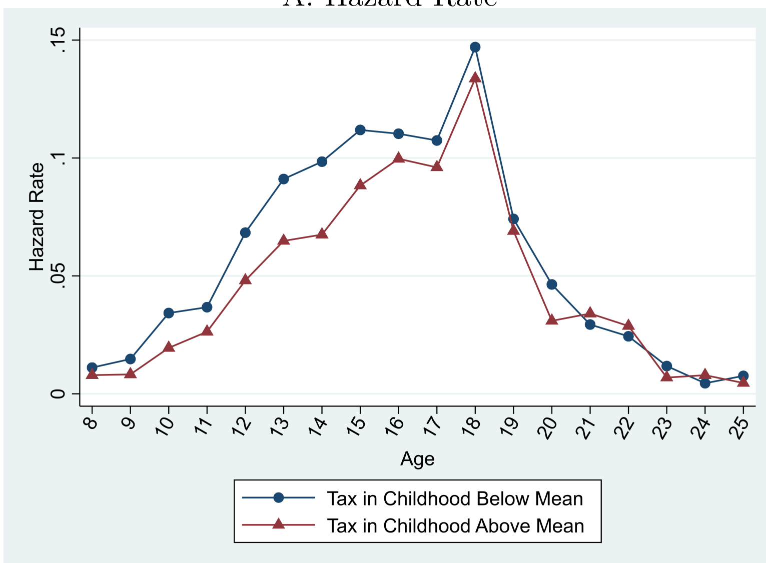
Table 2.2: Summary Statistics

	(1) Full Sample	(2) Mother Ever Smoked	(3) Mother Never Smoked
A. Individual Level			
Initiated in Sample	0.58	0.66	0.48
Left Sample Without Initiating	0.42	0.34	0.52
Average Cigarette Tax ($): Birth to Age 7	0.45	0.45	0.46
Hispanic	0.08	0.07	0.09
Black	0.17	0.16	0.18
Other Race (Including White)	0.75	0.77	0.73
Male	0.51	0.51	0.51
Mother's Age at Birth	26.48	26.02	27.22
Birth Order	1.95	1.96	1.93
Mother Ever Smoked	0.59	1.00	0.00
Parent Education: Less Than High School	0.03	0.04	0.02
Parent Education: High School	0.27	0.31	0.20
Parent Education: Some College	0.49	0.48	0.49
Parent Education: BA or More	0.21	0.17	0.28
Family Income: 1st Quartile	0.18	0.23	0.11
Family Income: 2nd Quartile	0.23	0.26	0.19
Family Income: 3rd Quartile	0.32	0.30	0.35
Family Income: 4th Quartile	0.27	0.21	0.35
Individuals	8,228	4,642	3,537
B. Individual-Age Level			
Current Cigarette Tax ($)	0.87	0.86	0.89
Smoking Initiation Hazard	0.054	0.065	0.041
Observations	89,289	47,330	41,910

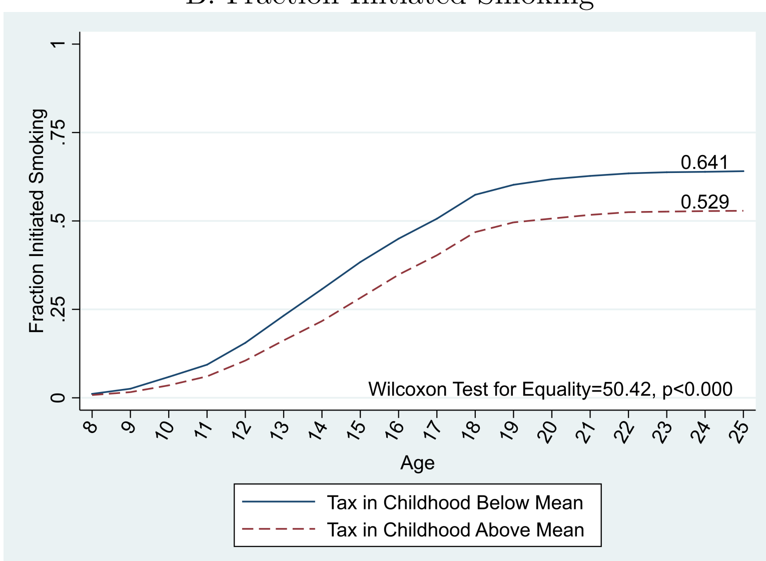
Notes: Means of each variable are reported. Data from the NLSCYA and weighted using NLSY79 weights for the mothers of those in our sample. Years of analysis range from 1984 to 2014. Income quartiles are the average quartile of total family income across sample years. Cigarette taxes are in real 2014 dollars.

Figure 2.4: Smoking Initiation Hazard Above and Below Mean Cigarette Tax in Childhood

A. Hazard Rate



B. Fraction Initiated Smoking



Notes: The hazard rate is the number of initiations at the given age divided by the number of individuals remaining in the sample. Panel B shows the fraction of of individuals we originally observe at age 8 who have initiated by a given age.

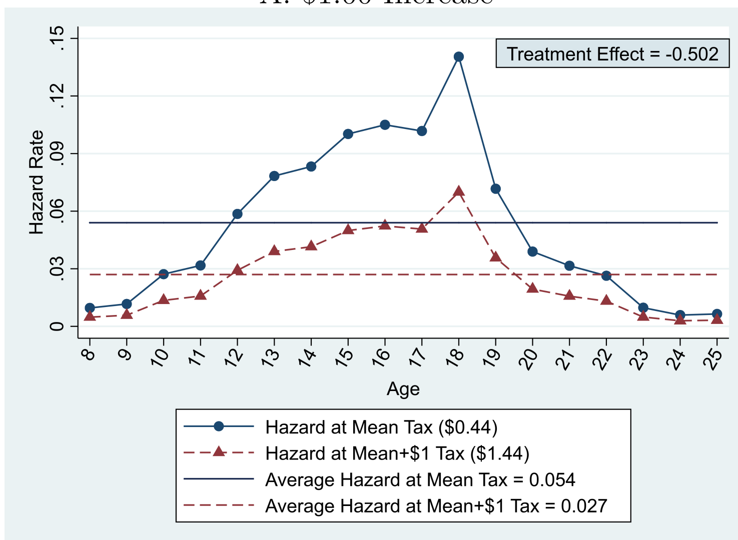
Table 2.3: Discrete-Time Hazard Model of Smoking Initiation

	(1) Full Sample	(2) Mother Ever Smoked	(3) Mother Never Smoked
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)			
Average Cigarette Tax ($): Birth to Age 7	0.498**	0.449**	0.566**
	(0.080)	(0.087)	(0.122)
Current Cigarette Tax ($)	1.062	1.023	1.139
	(0.053)	(0.060)	(0.103)
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)			
Average Cigarette Tax ($): Birth to Age 7	-0.125**	-0.138**	-0.109**
	(0.020)	(0.022)	(0.031)
Current Cigarette Tax ($)	0.015	0.006	0.035
	(0.013)	(0.015)	(0.026)
Mean Smoking Initiation Hazard	0.054	0.065	0.041
Individuals	8,228	4,642	3,537
Observations	89,289	47,330	41,910

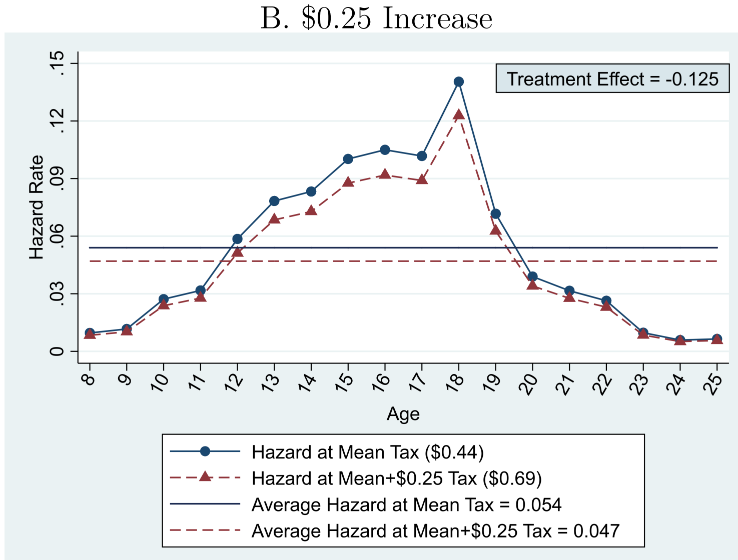
Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. Coefficients are estimated with a complementary log-log regression. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Figure 2.5: Effect of Cigarette Tax in Childhood on Smoking Initiation Hazard

A. $1.00 Increase



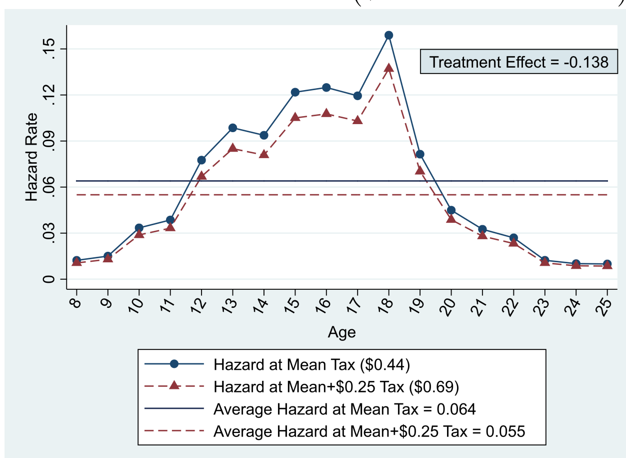
B. $0.25 Increase



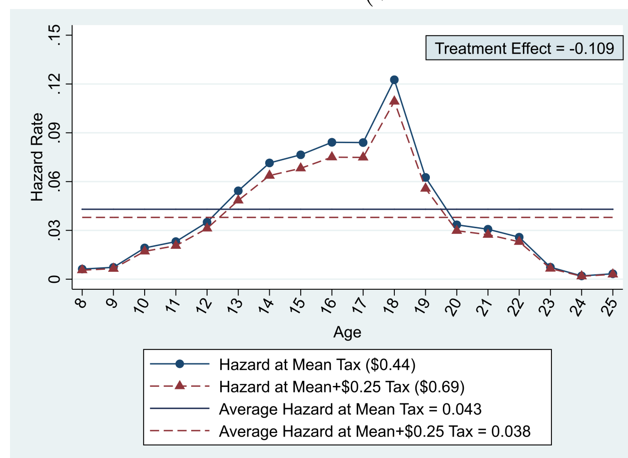
Notes: Results based on data from the NLSY Children and Young Adults (NLSCYA) in a discrete-time hazard model controlling for age, state, and year fixed effects as well as sex, race, parent education, family income, mother's age at birth, birth order, and whether mother ever smoked. A 12.5 percent decrease in the hazard of smoking initiation corresponds to a 0.7 percentage point decrease off a base of 5.4 percent.

Figure 2.6: Effect of Increased Cigarette Tax in Childhood on Smoking Initiation Hazard by Mother Smoking History

A. Mother Ever Smoked ($0.25 Tax Increase)



B. Mother Never Smoked ($0.25 Tax Increase)



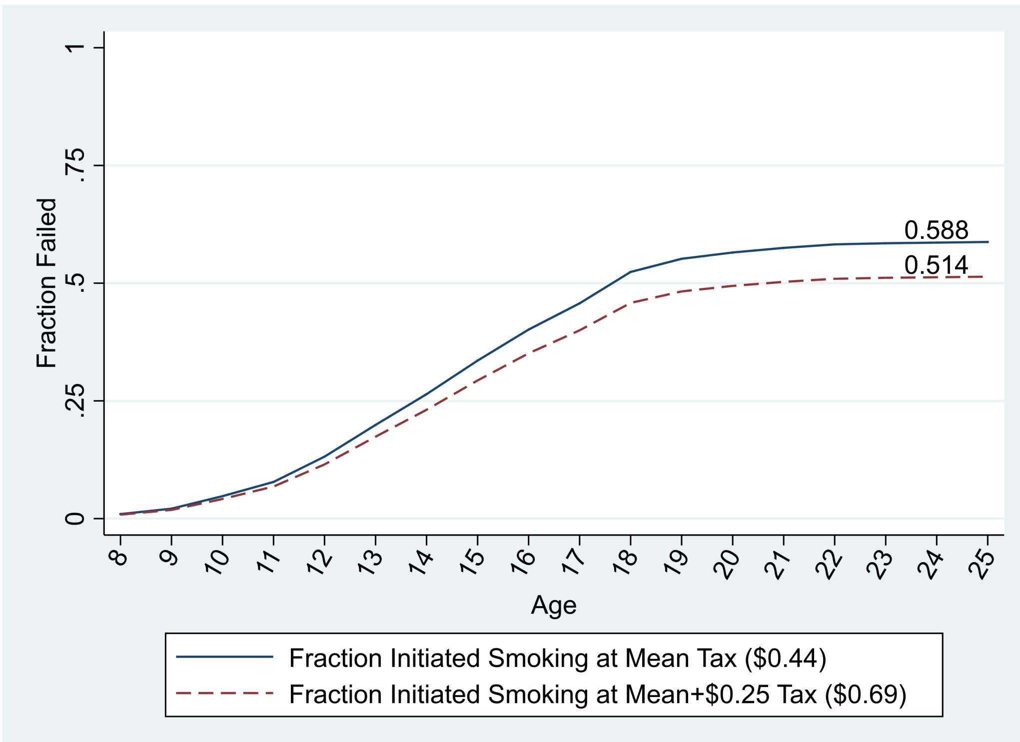
Notes: Results based on data from the NLSY Children and Young Adults (NLSCYA) in a discrete-time hazard model controlling for age, state, and year fixed effects, sex, race, parent education, family income, mother's age at birth, and birth order. Treatment effects are a decrease of 0.95 percentage points if the mother ever smoked and 0.48 percentage points if the mother never smoked.

Table 2.4: Discrete-time Hazard Model of Smoking Initiation, Current Tax and Tax in Childhood Separately

	(1)	(2)	(3)	(4)
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)				
Cigarette Tax in Childhood				0.519**
				(0.152)
Current Cigarette Tax	0.966	0.935**	1.014	
	(0.039)	(0.027)	(0.044)	
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)				
Cigarette Tax in Childhood				-0.120**
				(0.020)
Current Cigarette Tax	-0.009	-0.016**	0.004	
	(0.009)	(0.006)	(0.011)	
Demographics		X	X	X
State Fixed Effects			X	X
Individuals	8,229	8,228	8,228	8,228
Observations	89,305	89,299	89,299	89,358

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. Demographic controls include sex, race, parent education, mother's age at birth, birth order, mother smoking history, family income, and age and year fixed effects. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Figure 2.7: Baseline and Treated Fraction Initiated



Notes: The solid blue line is evaluated with variables at their mean value and the red dashed line is evaluated with a $0.25 higher cigarette tax during childhood and all other variables at their means.

Table 2.5: Discrete-Time Hazard Model of Smoking Initiation, Cigarette Taxes In Utero

	(1) Full Sample
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)	
Cigarette Tax in Utero	0.692
	(0.275)
Cigarette Tax in Childhood	0.788
	(0.235)
Current Cigarette Tax	1.078
	(0.056)
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)	
Cigarette Tax in Utero	-0.077
	(0.069)
Cigarette Tax in Childhood	-0.053
	(0.059)
Current Cigarette Tax	0.019
	(0.014)
Mean Smoking Initiation Hazard	0.053
Individuals	7,610
Observations	82,862

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

CHAPTER 3

TRADITIONAL PUBLIC SCHOOL DISTRICT RESOURCES AND CHARTER SCHOOL COMPETITION

3.1 Introduction

Starting in the early 1990s, some states allow a private education firm to make an agreement (called a charter) with the state government to provide education services separate from the traditional public school (TPS) system. The number of public school pupils attending charter schools nearly quadrupled from 2000 to 2013, and roughly 5 percent of students in the United States attend a charter school (Epple, Romano, and Zimmer, 2016). Charter school quality, in terms of outcomes of charter school attendees, has been the subject of heated debate and studies find heterogeneous effects (Zimmer et al., 2009; Angrist, Pathak, and Walters, 2013). If the presence of charter schools impacts the funding or quality of TPSs, then this indirect effect may have a larger impact on public school students in the United States, given the relatively small share of students attending charters.

When a student leaves a TPS to attend a charter school often the funding intended for that student follows them. This provides a competitive framework where schools are competing over students and the resources that come with them. A growing body of literature explores the effect of charter school expansion on TPS district finances and student achievement (Arsen and Ni, 2012; Cordes, 2017; Terrier and Ridley, 2018). Studies show that charter schools impact the composition of students attending TPSs (Booker, Zimmer, and Buddin, 2005) and house prices

(Andreyeva and Patrick, 2017; Brehm, Imberman, and Naretta, 2017; Cook, 2018). One topic that has recieved less attention is how changes in TPS spending influences their competition with charter schools. For example, Sullivan, Campbell, and Kisida (2008) provide qualitiative evidence that, in Washington D.C., "most of the changes that schools are making in order to attract more students have more to do with services for parents and the image of the school than with improving the educational attainment of students." Cook (2018) also finds that TPS districts allocate away from instructional and other spending in favor of new construction in the face of increased charter competition. Little is known about whether these behaviors are effective in attracting more students to remain in TPSs.

We address this gap by exploring whether increased resources for TPS districts helps stem the flow of students to charter schools in Ohio. To do this, we separately examine the impact of school resource expansions for capital projects and those for general purposes (current operating expenses, instructional expenditures, support services, etc.) on the fraction of potential TPS students attending a charter school, district outcomes, and home values. Our empirical strategy compares districts that narrowly pass a property tax levy to those that narrowly fail in a regression discontinuity framework that allows us to estimate the causal effect of increasing school resources. The primary assumption is that the only thing that changes at the 50 percent vote threshold is the amount of resources from the levy and not any other district characteristics. We find that districts that pass capital levies subsequently increase capital spending and districts that pass general purpose levies increase spending across all categories. Both type of resource expansions protect TPS districts from losing students to digital charters, but we find no effect for students lost to brick-and-mortar charters. Specifically, passing a capital levy decreases the fraction of potential students who attend a digital charter school by 0.20 percentage points (20 percent from a base of 0.1) and passing a general purpose levy decreases this fraction by 0.15 percentage points (12.5 percent from a base of 0.12). We do not find any evidence that increased resources improves student test scores, but we are unable to rule out reasonably large effects. Since both capital and general levies lead to increased capital spending the key difference between them is spending on current expenses such as teachers. General purpose levies are used in part to hire additional teachers, which decreases the student-teacher ratio by about 27 percentage points (1.5 percent from a base of 17.52). This investment in teachers is partially capitalized and increases home values by 0.48 to 0.79 percent, although these estimates are imprecise. In contrast, capital levies crowd out spending on teachers such that the student-teacher ratio increases and home values subsequently decrease. Specifically, we find that passing a capital levy decreases home values between 1.5 and 1.75 percent. This could be in response to the increase in the student-teacher ratio but may also be related to the disruption from increased construction activity. In all, this suggests that although either type of investment makes TPS districts more competitive with digital charters, increasing the general operating budget improves school quality in a way that is valued by residents in the districts, while capital investments may be a more superficial way to attract would-be transfers to charter schools.

This paper makes important contributions to several strands of literature. Our main contribution is to the literature that assesses the effect of TPS district behavior on their competition with charter schools. It is important to consider how districts respond to charter competition to understand the general equilibrium effects of charter school expansion. Cook (2018) finds that TPS districts spend less on instructional expenditures and more on new construction capital outlays in response to charter competition. We find a larger impact of capital levies on retaining students than general purpose levies. However, general purpose levies also improve other measures of school inputs and quality. This suggests that the strategy observed in Cook (2018) where districts spend more on capital projects may be effective at keeping students from transferring to digital charter schools but not brick-and-mortar charter schools. Digital charters are available to all students in the state, while competition from brick-and-mortar charters is more limited to schools in the same local geographic area. This may explain why we detect effects for students lost to digital charters because their competition with TPS districts is uniform across the state while the degree of brick-and-mortar competition varies.

Second, we contribute to the literature estimating the effect of school spending on education production. There is growing evidence that increased school spending improves student outcomes in terms of labor market participation and earnings (Jackson, Johnson, and Persico, 2016), test scores in low-income districts relative to high-income districts (Lafortune, Rothstein, and Schanzenbach, 2018), and average test scores and graduation rates (Miller, 2018). There is also evidence from California and Texas that increased capital spending from approving bond levies has no meaningful effect on test scores (Martorell, Stange, and McFarlin, 2016) but is capitalized into house prices (Cellini, Ferreira, and Rothstein, 2010). Our work is most closely related to the concurrent work of Enami (2018), who estimates the effect of passing various types of levies in Ohio on student achievement. Enami (2018) finds a small positive effect of increased operating expenditures on math proficiency rates among those subject to Ohio graduation tests. We find no significant effect of increases school resources on student achievement, but we are unable to rule out positive effects of up to 0.06 standard deviations, which would allow for the size of effects found in Enami (2018), but would rule out some of the larger estimates in the literature. Our results underscore the importance of considering not just the amount of resources but how they are allocated in estimating the effect of school spending on student achievement.

Third, we contribute to the literature on how district expenditures are capitalized into housing values. An extensive literature finds that school quality is reflected in local housing prices (Nguyen-Hoang and Yinger, 2011). A smaller literature assesses whether school spending is capitalized into housing values regardless of the impact on school quality.[1] Recently, Cellini, Ferreira, and Rothstein (2010) used property tax levy referenda in California and find that capital investments have a large causal impact on home prices. We provide separate estimates of capital investment and other school spending on residential property values in Ohio. We find that increasing instructional spending by hiring more teachers leads to increased home prices, while crowding out instructional spending by investing more in capital leads to decreased home values. This differs from what previous work finds for capital spending. One

[1] Hilber and Mayer (2009), Mathur (2008), Brasington and Haurin (2006), Crone (2006), Barrow and Rouse (2004), Beron, Murdoch, and Thayer (2001), Downes and Zabel (2002), Black (1999), and Brasington (1999) all measure school quality (for at least some of their analysis) based solely on per-pupil expenditures.

key difference in our context that can reconcile these results is that capital levies crowd out can crowd out general purpose levies in Ohio, but California's funding for instructional spending is from an entirely separate system that is insulated from this crowd out.

Finally, we contribute to the literature assessing the value parents place on various measures of school quality. The literature is mixed, with some finding that test scores dominate satisfaction with the learning environment or student happiness at school (Hastings and Weinstein, 2008; Gibbons and Silva, 2011) while others show that parents request teachers that promote student satisfaction and put less weight on the teacher's ability to increase standardized testing outcomes (Jacob, 2007). Imberman and Lovenheim (2016) show that the release of value-added measures has no impact on house prices, suggesting that parents do not value value-added. We add to this literature by testing whether various types of expenditures affect education production through test scores or student/parent satisfaction through student transfers to charter schools. We find that increased spending from passing levies increases the competitiveness of TPS districts with digital charter schools, but find no evidence that test scores improve. This supports the argument that parents respond to the environment of the school more than test scores, which may be due to salience or reflect parent preferences.

Our findings also have several important implications for policy. First, TPS districts become more competitive with digital charter schools by investing in capital even without improving test scores or other measures of school quality. This suggests that the strategy documented in Cook (2018), where districts divert resources from instruction to capital spending in response to increased charter competition, is effective at retaining students. However, it is not clear whether keeping students in the TPS makes students better or worse off without knowing the relative quality of the digital charter and the TPS. Additionally, it seems that investing in capital at the expense of instructional spending decreases home values, which may reflect other dimensions of how parents and others in the community value different types of spending. Second, districts are less likely to pass additional future levies after a successful referendum so if administrators choose to increase spending on non-productive inputs to please parents, then it likely crowds out spending on more productive inputs in the future.

The next section provides background information on Ohio elections, school finance, and charter schools with Section 3.2.2 detailing the process of proposing capital bond referenda and Section 3.2.3 discussing charter schools in Ohio. Section 3.3 details the sources of data for this project and discusses each variable used in the analysis. Section 3.4 discusses our empirical strategy. Section 3.5 reports our results and Section 3.6 concludes.

3.2 Background Information

3.2.1 Property Tax Levy Referendum

We consider local referenda for property tax increases in Ohio. The Constitution of Ohio requires that property taxes in excess of 10 mills (a mill is one thousandth of a dollar) be approved via local referendum (Ohio Const. Article XII §2). In

1976, the Ohio legislature passed House Bill 920, which automatically adjusts the millage rate when housing valuations increase to maintain the original dollar value approved or value implied by the original millage rate. This "inflation adjustment factor" results in Ohio school districts proposing around two to three hundred levies per year, a relatively high number compared with other states. School districts are permitted to propose levies in November general elections, primary elections in March or May, or special elections held in February or August, up to four elections per year. The majority of levies are proposed in general elections. Districts can also propose multiple levies for distinct purposes in each election.

Nearly half of the revenue for Ohio public schools comes from local sources, which is predominately property tax revenue. The permissible uses for property tax levies include current operating expenses such as teacher and administrator salaries, general ongoing improvements, specific permanent improvements, purchase of educational technology, or debt service for bonds issued for school construction. Districts that seek to propose a levy follow a set script given by the Secretary of State where they fill in the information for their levy. For example:

> An additional tax for the benefit of the Delphos City School District for the purpose of **CURRENT EXPENSES** at a rate not exceeding 1.46 mills for each one dollar of valuation, which amounts to $0.146 for each one hundred dollars of valuation, for a period of 5 years, commencing in 2009, first due in calendar year 2010?

The purposes, which include current expenses, emergency requirements, permanent improvements, the avoid and operating deficit, and others are displayed in bold on the ballots.

3.2.2 Capital Bond Referendum

Bond authorization for new construction projects and capital improvements is a two-stage process. First, prior to proposing a new capital bond through local referendum, districts must be issued approval from the Ohio School Facilities Commission (Ohio Revised Code 3318.06 Section A).[2] Approval depends on the amount of state aid that would be required as well as the amount of need in the district. A key measure used by the facilities commission is the adjusted valuation per-pupil (see Ohio Revised Code 3318.011), calculated by taking the total valuation of all district property and dividing by fall enrollment counts. Districts with the lowest adjusted valuation per-pupil are most likely to receive approval.

Second, upon receiving state approval, districts must pass a ballot measure in a local election to authorize the district to take out a capital bond. Proposals require a simple majority to pass. On average, Ohio has around one to two hundred new capital proposals in a given year,[3] about half of which are approved. Passage of a bond proposal authorizes an increase in the millage rate estimated by the county auditor to pay off the principal and interest within the proposed time frame.

[2] Arizona is the only other state requiring state approval prior to proposing a capital bond through local referendum.

[3] Thus the combined number of bonds and levies in a given year is usually around four to five hundred.

3.2.3 Charter Schools

In the United States, charter schools are independently run educational organizations that declare and sign a "charter" containing their structure and outlining detailed plans for achieving student success. The Ohio Legislature approved Ohio's charter school law in 1997. In Ohio, while students are only permitted to attend a TPS based on the geographic location of their residence, students across the state are able to attend any charter school they desire.[4] When a student transfers to a charter from a public school, the public funding follows the child. Thus, charters embrace the essence of a competitive market where the students are the customers. Any charter failing to attract the number of students needed to fund operating costs must eventually shut down.

Charters also differ from public schools in that they may be shut down if the Ohio Department of Education (ODE) deems that they are not following the goals and requirements of their charter. Another important difference unique to Ohio is that charter schools lack a sports team and public schools prohibit charter student athletic participation. Anecdotal conversations with Ohio administrators pointed to this as one of the largest factors deterring families from sending their child to a charter school.

Ohio charter schools are categorized as either a traditional "brick-and-mortar" or "digital" charter. The main difference between these two types of charter schools is that digital charters provide all instruction online and are required to provide each

[4] Local school districts are required to provide transportation to any student living more than two miles away from their desired charter school as long as the charter is no further than 30 minutes away from the school of residence.

student with a laptop. Around 78,500 out of the 1,682,031 public school students (about 4.7 percent) in Ohio attend a brick-and-mortar charter school as of 2017. Ohio has the second-largest (after Arizona) online charter presence with over 30,000 students enrolled, or around 2 percent of public school students, in a digital charter. While some digital charter schools limit enrollment to district residents only, a vast majority of digital charters allow students from across the state to enroll.

3.3 Data

We combine data on Ohio school districts from multiple sources. Table 3.1 lists each of these sources, the relevant variables, and the years for which data is available. Election data is available from the Office of the Ohio Secretary of State. Data on Ohio school district characteristics is available from the the National Center for Education Statistics' Common Core of Data (CCD), the Ohio Department of Education (ODE), and the Ohio Department of Taxation (DOT).

3.3.1 Property Tax Referenda

The Office of the Ohio Secretary of State collects and stores data about all local, state, and federal elections from the Board of Elections in each county. We obtained information for all local referenda from 1996 to 2012. For each election, we observe the election date, the number of votes in favor of the proposition, the number of votes against, the dollar amount or millage rate of the proposed property tax[5], and

[5]Ballots for tax levies reflect the proposed millage rate and those for bond levies reflect the dollar amount of the capital project.

the purpose of the tax.

We separate levies into six categories based on the text of their stated purpose. These categories include: current expenses, emergency, permanent improvements, construction, facilities, and to avoid an operating deficit. Figure 3.1 shows the frequency of the different types of levies in our data and differentiates between what we are calling capital levies and general levies. The most common purposes are for current expenses and emergency requirements, followed by permanent improvements. Construction levies are often paired with other purposes. Levies for construction, facilities, or permanent improvements, as well as all capital bonds, are considered capital levies. Levies are considered "general levies" if they are categorized as current expenses, emergency purposes, or to avoid an operating deficit.

The results of local property tax referenda are summarized by year in Table 3.2, separately for capital and general levies. This table reports the number of measures, average proposed levy amount per pupil, and the fraction approved. The dollar amount of capital levies tends to be larger than for general levies and the fraction approved is generally between 50 and 70. Figure 3.2 describes the distribution of vote share for capital and general levies. General levies are slightly more common than capital levies and the vote share for both types of levies are more or less normally distributed with some excess mass to the right of 50 percent in favor. This excess mass reflects selection into introducing levies. That is, districts might not put a levy on the ballot if they think it will fail, so the set of levies proposed will be more likely to pass. We will show that this bunching is minor and satisfies standard density tests that show districts can influence, but not perfectly control, the final vote share for their levies.

Passing a levy also impacts the likelihood of passing levies in the future. To shed light on the expected values for π_h weights used to convert ITT estimates to TOT, we display the change in the probability of passing a capital or general levy after a levy is passed in Figure 3.3. Panel A shows what happens after a general levy is passed and Panel B is after a capital levy is passed. Passing a general levy decreases the probability of passing another general levy for the 3 years following the election. The probability becomes positive in the 4 to 6 years after with a notable bump at 5 years. This is indicative of a 5-year term for many general purpose levies. Capital levies are slightly less likely to be passed in years that general levies are passed, then the probability is near zero in the following years. Passing a capital levy decreases the probability of passing other capital levies for up to four years following the election, then there is a 5-year bump that is smaller than for general levies.

Figure 3.4 shows similar probabilities after a levy is defeated. Notice that the predicted probabilities are not exactly zero in the year the levy is defeated because districts can proposed additional levies in that year. So a coefficient of 0.05 suggests that 5 percent of districts that propose a levy that fails will pass another levy in one of the later elections that year. Notably, there is a 20 to 25 percent chance of passing a general levy the year after one is defeated and a 15 to 20 percent chance of passing a capital levy the year after a capital levy fails. This suggests that districts that narrowly fail to pass a levy will likely "catch up" in the years following, which would make the ITT effects an underestimate of the effect of increased spending after levy passage. Overall, there are clear dynamics in the timing of passing levies that underscore the importance of the dynamic regression discontinuity method we use.

3.3.2 District Finances and Student Characteristics

The CCD includes district revenue, expenditures, teacher counts, and student counts by subgroups, which include race, free or reduced price lunch eligibility, and special education. We add student outcomes from the ODE that includes district value-added, a performance index, and the number of students who transfer to a charter school. We also include assessed property values for each school district from the DOT.

Summary statistics for these measures at the district-year level are reported in Table 3.3 for Ohio's 613 school districts. The first column shows means for all district-years, means for districts in the year prior to a levy win are in the second column, and the third column provides means for districts in the year prior to a levy loss. The final column shows the difference between columns 2 and 3 and provides a p-value for the difference. Districts in Ohio spend over $10,000 per pupil. This is divided into per pupil expenditures of about $1,200 on capital outlays, $5,000 on instruction, and $3,000 on support services. About 1.7 percent of potential TPS students attend a charter school with 1.1 percent attending a digital charter and 0.8 percent attending brick-and-mortar charter. The average number of students per district is about 3,000 and the average number of teachers is 760. The only significant differences between districts that fail and those that pass a levy the year before an election are small differences in capital spending per pupil and students lost to digital charters. We show later that these pre-treatment differences disappear once we add basic controls.

The lack of pre-treatment differences supports our identification assumptions.

One difficulty in combining information from multiple sources is lining up the years of observation. That is, district information is measured by school year or fiscal year which runs from July 1 of year t to June 30 of $t+1$ in the ODE and CCD data. Alternatively, election results and DOT data relate to the tax year or calendar year. This becomes important for interpreting the timing of results. We therefore treat the year of election is a half-treated year. Specifically, the indicator for a passed levy is set to 0 if the levy failed and in the time prior to the passage of the levy, it is set to 1/2 in the year the levy was passed, and set to 1 in subsequent years.

3.4 Empirical Strategy

We use a dynamic regression discontinuity design developed by Cellini, Ferreira, and Rothstein (2010), which allows for both the possibility of repeated treatments and lagged outcomes. This design addresses the fact that although each election is a sharp RD, districts can propose additional levies after the outcome and the impact of passing a levy may not be felt for several years. They distinguish between two types of treatment effects. The intent-to-treat (ITT) effects are the effect of passing a levy including any post-election behavior of districts, including proposing additional levies. The treatment-on-the-treated (TOT) effects isolate the effect of a single levy regardless of all other referenda including other proposed levies and their outcomes. Both effects are policy relevant but contain slightly different information. The ITT effects are how much better off a district that narrowly passes a levy is than a district that narrowly fails to pass a levy, while the TOT is the causal effect of the increased spending.

3.4.1 Intent-to-Treat Effects (ITT)

We estimate the ITT effects with

$$y_{jt\tau} = \theta_\tau^{ITT} b_{jt} + P_g(\nu_{jt}, \gamma_\tau) + \alpha_\tau + \kappa_t + \lambda_{jt} + \varepsilon_{jt\tau} \tag{3.1}$$

where $y_{jt\tau}$ is a district j outcome occurring τ years since a focal election in year t, b_{jt} is an indicator for the passage of a levy by district j in year t, $P_g(\cdot)$ is a cubic in vote-share ν_{jt} with coefficients γ_τ. α_τ, κ_t, and λ_{jt} are relative-year, focal-year, and election fixed effects, respectively. We estimate outcome dynamics using 2 lag years and 6 lead years for each focal election[6] and cluster standard errors at the district level. We estimate this equation with data generated by taking each focal election in year t for district j and expanding the explanatory variables from (3.1) so that there is one observation for each year relative to the focal election.[7] Then we match the leads and lags of each outcome variable in relation to the given focal election. As a result, if a district proposes levies in quick succession, a given district-year outcome may appear as both a lead observation for the earlier focal election as well as a lag observation in the later focal election. This method is similar to simultaneously running a standard regression discontinuity for a focal election in

[6] Both γ_τ and θ_τ^{ITT} are allowed to vary freely for $\tau \geq 0$ but are constrained to be 0 for $\tau < 0$.

[7] Because we use 2 lags and 6 leads, we expand the dataset so that each focal election has 9 observations.

year t on outcomes in year $t + \tau$. However, stacking each of these regressions and then clustering by district allows the researcher to obtain more efficient estimates that account for autocorrelation across effect dynamics within a district.

3.4.2 Treatment-on-the-Treated Effects (TOT)

We estimate TOT effects using a recursive strategy from Cellini, Ferreira, and Rothstein (2010). The recursive strategy aggregates weighted ITT effects. Specifically,

$$\theta_\tau^{TOT} = \theta_\tau^{ITT} - \sum_{h=1}^{\tau} \pi_h \theta_{\tau-h}^{TOT}, \tag{3.2}$$

where $\pi_t \equiv \frac{\partial b_{j,t-\tau+h}}{\partial b_{j,t-\tau}}$ is the effect of authorizing a levy on the probability of authorizing another levy h years later. Also, note that $\theta_0^{ITT} = \theta_0^{TOT}$. Standard errors are calculated using the delta method.[8]

3.4.3 Total Effects

In addition to the dynamic ITT and TOT estimates outlined in Equation 3.1 and Equation 3.2, we estimate the total effect of passing a levy over the 6 years following the election. To do this, we sum the individual θ_τ coefficients for $\tau \geq 0$ and calculate standard errors via the delta method. These estimates provide a clear summary of policy effects of the levy passage that are more easily compared to estimates using other identification strategies.

[8]Equation 3.2 becomes imprecise with long lags, so Cellini, Ferreira, and Rothstein (2010) propose another estimator that improves efficiency and precision when the lags are long. Since we are focusing on relatively short lags (only up to 6 years), we use the recursive strategy as our main TOT estimates.

3.4.4 Identification Checks

The sufficient conditions for identification in a regression discontinuity design are continuity of the conditional expectation of counterfactual outcomes in the running variable. This condition is generally violated when (1) the treatment assignment rule (cutoff) is public knowledge and (2) the running variable (vote share) is manipulable. The first condition is certainly true in our context, but the second may or may not be a threat to identification. McCrary (2008) distinguishes between "complete" and "partial" manipulation. Manipulation is "complete" when an agent can perfectly determine the value of their running variable. Manipulation is "partial" when agents can impact the value of the running variable by their behavior, but there still remains an idiosyncratic element. "Complete" manipulation usually poses a problem for identification whereas "partial" manipulation does not.

Districts clearly influence the vote share a proposed levies receives by setting the characteristics of the levy. For example, Figure 3.5 shows a negative relationship between vote share and the dollar amount of the levy above the cutoff. There is no relationship between levy size and vote share below the cutoff. This suggests that districts choose a levy with the highest dollar amount that will still pass. However, this is only a threat to our identification strategy if districts are able to perfectly determine the vote share. We claim that even if districts influence the votes they cannot directly manipulate the vote share.

We perform the test suggested by McCrary (2008) for detecting manipulation of the running variable. The results of this test are reported graphically in Figure 3.6 with estimates for the discontinuity in the density at the threshold reported in

Table 3.4. We fail to reject continuity in the distribution of the vote share across the cut off for both capital and general levy elections. There may be concern that some of the elections throughout the year, especially the special elections that are not held in conjunction with the primary or general election schedule may be easier for districts to manipulate. We perform additional McCrary tests for subsamples based on the month of the election or proposed length of the levy and still fail to reject the null hypothesis of continuity in the distribution of the vote share, which provides additional evidence that districts are unable to perfectly manipulate the vote share.

We also plot averages by vote share in the periods preceding and following elections for an exhaustive list of district characteristics to visually check that predetermined variables are not effected by the treatment assignment. Figure 3.7 displays the average fraction of students lost to digital charter schools by vote share in the year before, year of, and year following an election. Panel A shows the averages for capital levy elections and panel B shows averages for general levies. Also plotted is a quadratic fit to the averages and 95 percent confidence bands. There is no difference at the threshold in the year before either type of election. In the year of the election, the average is slightly lower to the right of the threshold for general purpose levies but not capital levies. In the year after the election the drop to the right of the threshold is larger for general levies and small for capital levies. This suggests that any difference in the fraction of students lost to digital charter schools observed is not due to pre-election differences in the outcome on either side of the threshold, which supports our identification assumptions. [9]

We verify the visual inspection of these trends with formal estimates and hypothesis tests in Table 3.5. Columns 1 and 2 are differences in characteristics in the year before an election for capital levies and general purpose levies, respectively. The estimates in column 3 and 4 are the difference from two years prior to one year prior to the election. There are no significant differences in any of these characteristics. There is a marginally significant difference in the fraction of students that are black from two to one year prior to the election, but the estimate is small in magnitude. This provides further evidence that districts above and below the 50 percent vote share threshold are not systematically different, which supports the assumptions necessary for interpreting our results as causal.

3.5 Results

3.5.1 School Finances

We begin with estimates of how levy passage affects district spending following an election. Figure 3.8 presents graphical analysis of district spending by vote share relative to the 50 percent threshold in the year before and three years after the election. The averages are shown in bins of two percentage points and are net of calendar year effects and are relative to the first bin to the left of the threshold. The

[9] Similar balance graphs are available for other district characteristics in Figure C.3 through Figure C.15 and show no difference at the threshold prior to the election for instructional expenditures, support service expenditures, fraction of students lost to a charter school, residential values, the performance index, district value-added, fraction meeting annual yearly progress, student-teacher ratio, number of students, fraction of black students, or fraction of students eligible for free lunch.

left side of panel A shows no discontinuity across the threshold for capital spending in the year before a capital levy election, but there is a jump of about 2,000 dollars per pupil in capital spending at the 50 percent threshold three years after a capital levy election. On the right side of panel A, average capital spending is slightly less to the right of the threshold for general purpose levy elections. Rather than a jump, this appears to be the continuation of a negative relationship between vote share and spending, which suggests that voters are less likely to approve a general levy in districts that are already spending more on capital. Three years following a general levy election, capital spending is flat across all vote shares, which suggests that initially lower capital-spending districts catch up after passing a general levy although there is no visible discontinuity. Graphs for instructional spending in panel B are slightly more noisy, but also exhibit no discontinuity at the 50 percent vote share threshold in the year prior to the election for either capital (on the left) or general levies (on the right). Average instructional spending to the right of the threshold is consistently higher three years after a general levy election but not for capital elections. Taken together, these graphs confirm no pre-election discontinuity at the 50 percent threshold. Also, capital spending increases after passing a capital levy and instructional spending increases after passing a general purpose levy.[10]

Figure 3.9 shows the effect of each type of levy on capital and instructional spending using the ITT method described in Equation 3.1 in panel A and the TOT method described in Equation 3.2 in panel B. Panel A shows that passing a capital levy increases capital spending a modest amount in the year of and following the

[10]In the appendix, Figure C.1 shows a similar relationship between vote share and support service spending, but the relationship is even noisier than for instructional spending.

election then by about $1,000 per pupil in the second year and up to about $1,500 per pupil in the third year. After the third year, spending drops down close to zero and is significantly negative in the fifth year for the ITT. This is consistent with the timeline of most capital projects. Projects are implemented in the few years following the passage of the levy, then spending returns to lower levels once the project is finished. Additionally, "control" districts, whose levy failed previously, catch up as they propose and pass new levies. Passing a general levy also provides a modest boost to capital spending, with significant ITT effects in the year of and immediately following the election and consistently positive TOT effects between about $500 and $1,000 per pupil. Panel B shows that capital levies do not increase instructional spending, but there is a persistent increase in instructional spending between $100 and $200 per pupil after passage of a general purpose levy.

These relationships are summarized as total effects in Table 3.6. The ITT effects are smaller in magnitude and less precisely estimated than the TOT effects. Because the TOT effects are a more closely related to the actual changes in spending that occur after passing a levy we focus on those magnitudes. In the 6.5 years following capital levy passage, districts spend $3,668 per pupil more on capital and slightly less on instruction and support services. By dividing by 6.5, we get that the average yearly increase in capital spending is $564 per pupil or a 39 percent increase in spending. Following a successful general levy referendum, per-pupil spending increases by $3,553 for capital, $766 for instruction, and $232 for support services. These correspond to annual effects of $546, $188, and $36; or 48%, 2.2%, and 1%, respectively. In sum, a given levy election has a significant and meaningful impact on school district finances, especially in capital spending.

3.5.2 Charter Competition

Figure 3.10 displays the average fraction of students lost to charter schools, by vote share. These estimates are noisy, but show no systematic jump in the average at the 50 percent threshold. Figure 3.11 shows the effect of passing a levy on the fraction of students lost to charter schools, with estimates for digital charters in panel A and estimates for brick-and-mortar charters in panel B. For digital charters, the ITT estimates on the left are imprecise and negative after passing either type of levy, although slightly larger magnitude for capital levies. The estimates are nearer to zero in the fifth and sixth year following the election. The TOT estimates on the right show a consistent downward trend for both general and capital levies, with a more pronounced effect for capital levies. Similar estimates for the fraction of students lost to brick-and-mortar charters in panel B show that the effects are near zero and imprecisely estimated.

These effects are summarized in Table 3.7 as the total effect of passing a levy on the fraction of potential students attending charters over the following 6.5 years (the year of the election is a half-treated year). The effect for brick-and-mortar charters is slightly positive and not statistically significant. The effect for digital charters is negative for both the ITT and TOT estimates with larger effects in the TOT estimates, which is consistent with the dynamic estimates. The total TOT effect of passing a capital levy is 1.3 percentage points fewer students lost to digital charters. The total TOT effect for general levies is 1 percentage point. These estimates are based on all elections, including districts with multiple elections in a year, but are robust to limiting attention to the first election of each year.[11]

We estimate the average yearly decrease in the fraction of potential students attending digital charters by dividing these estimates by 6.5 to get 0.2 percent points and 0.15 percentage points for capital and general levies, respectively. Taking into account the average fraction of students lost, these constitute decreases of 20 percent for capital levies and 12.5 percent for general levies. On average, TPS districts in Ohio lose around 40 students to digital charters, which amounts to about $232,650 in funding. Thus, passing a levy decreases the number of students lost to digital charters by about 2 for every thousand students. The amount of money saved for the average TPS district would be about $3,024.

3.5.3 Student and Teacher Counts

Next, we examine the effect of passing levies on student and teacher counts to see whether keeping students from attending digital charters increased the total number of students and whether districts used their expanded resources to increase the number of teachers. Figure 3.12 shows the raw average student and teacher counts by vote share. There is no discontinuity at the threshold for student or teacher counts before either a capital or general levy is passed. The other averages are noisy with a lot of movement across vote share, but no clear pattern emerges from this graphical evidence. To provide a clearer picture of what happens to student and teacher counts following levy passage, we show the annual DRD effects in Figure 3.13. Panel

[11] Estimates using just the first election of each year are available in Table C.4.

A shows the estimates for the number of students in the district. The ITT and TOT estimates for capital levies are all negative, but imprecisely estimated. The estimates for general levies are larger in magnitude, positive, and more precisely estimated with some effects significant at the 95 percent level. In panel B, the pattern for number of teachers following a capital levy is less consistent than for student counts with some positive estimates and some negative. Teacher counts increased following a general election with estimates between 5 and 10 additional teachers, many of which are statistically significant. Since both the number of students and teachers increase significantly following a general levy we also show what happens to the number of students per teacher in panel C. This shows that the number of students per teacher decreased significantly after passage of a general levy while the ratio may have increased following a capital levy although the estimates are not statistically significant.

To summarize, we show total effects of levy passage on student and teacher counts in Table 3.8. Our estimates suggest that following the passage of a capital levy, and consistent with the graphical evidence, the number of students and teachers both decreased and the student-to-teacher ratio increased. These estimates are quite small compared to the size of the standard errors, so we are also do not rule out that there was no effect on student or teacher counts. The estimates for general levies are more compelling and since the ITT and TOT estimates are quite similar we will focus on discussion on the TOT estimates. In the 6.5 years after passing a general purpose levy, districts have 403 additional students and 51 additional teachers, which decreased the student-to-teacher ratio by 1.74 students. By dividing by 6.5, these effects translate into 62 additional students each year being served by an additional 7.9 teachers. This amounts to an annual decrease in the number of students per teacher by 0.27. This suggests that passing general levies leads to investing in teachers while passing capital levies may crowd out resources that might have otherwise gone to teachers.

3.5.4 Residential Home Values

We next explore whether passing a levy is capitalized into residential values. Property values by vote share are graphed in Figure 3.14. These graphs do not show a consistent pattern at the threshold, but do suggest that property values are lower three years following a successful capital levy than the year prior. Figure 3.15 shows the effect of passing capital or general levies on log residential values in panel A and residential values in tens of thousands of dollars in panel B. For log residential values, the ITT estimates are slightly positive for general levies and slightly negative for capital levies. The TOT estimates show a similar pattern but with larger magnitudes that are more precisely estimated. For residential values in tens of thousands of dollars, the ITT estimates are positive by imprecisely estimated for both types of levy, but the TOT estimates for capital levies are increasingly negative over time.

These effects are summarized in Table 3.9. The TOT estimates suggest that passing a capital levy decreases residential values by 9.7 percent or 11.4 percent[12] in the 6.5 years following the election. For the 6.5 years after passing a general levy property values increase by 3.1 percent or 5.1 percent. Dividing by 6.5, these

[12]This is based on the estimate in tens of thousands of dollars because $\frac{-2{,}186}{19{,}268} \approx -0.114$.

estimates suggest that passing a capital levy decreases property values between 1.5 and 1.75 percent annually and passing a general purpose levy increases property values by 0.48 to 0.79 percent. The only estimate that is statistically significant is for log residential values after a capital levy, but these suggest that passing a capital levy decreases property values while passing a general purpose levy increases property values.

3.5.5 Student Outcomes

Finally, we explore the effect of levy passage on student outcomes in terms of the district performance index, value added, and the fraction of students meeting annual yearly progress standards. Figure 3.16 shows the average of these student outcomes by vote share the year before and three years after a capital or general levy election. These estimates do not show a clear pattern. Figure 3.17 shows the DRD estimates and again there is not much of a pattern. The summary estimates in Table 3.10 show that none of the estimates are statistically different from zero. For capital levies we can rule out a change in the performance index of more than 0.055 standard deviations or more than 0.063 standard deviations for general levies. These estimates are not precise enough to rule out meaningful positive or negative achievement effects.

3.6 Conclusion

In this paper, we estimate the effect of increased resources for traditional public schools on several district-level outcomes. Passing a capital levy increases capital outlays and passing a general purpose levy increases instructional, support service, and capital spending. Both types of levy improve the ability of districts to retain students who would have otherwise attended digital charter schools but had no meaningful impact on students lost to brick-and-mortar charter schools. Specifically, passing a capital levy decreases the fraction of potential students who attend digital charters each year by 20 percent and general levies decrease the fraction by 12.5 percent. We find no evidence that either type of levy has an effect on test scores, although the estimates are imprecise and we cannot rule out meaningful positive or negative effects. We also find some important heterogeneity between the effects of capital and general levies. General purpose levies increase the number of students in the district, but the number of teachers increases proportionately more so that the student-teacher ratio decreases. The estimates for capital levies are less precise, but shows an opposite effect where the number of students and teacher both decline and the student teacher ratio increases. Possibly due to this differential investment, home values increase after general levies but decrease after capital levies.

There are several limitations to the current study that can be improved upon by future research. First, we are unable to say precisely why competition with digital charters is more affected than with brick-and-mortar charters. It may be that digital charters are available to all students in the state, while competition from brick-and-mortar charters is more limited to schools in the same local geographic area. We lack to statistical power to explore the heterogeneity between areas based on the level of competition with brick-and-mortar charters, but additional years of data may allow future work to tease these out. Second, the outcome data available for test scores and housing values is not ideal. The residential values we use are district-level aggregates from the Department of Taxation and reflect assessed value rather than the sale price of homes. Thus, we are unable to distinguish between the value of individual homes or the total number of homes included in the aggregate estimate. To better measure the impact on capitalization, we would need additional information that more closely reflect changes in the average market value of property in the district. The district-level achievement measures we use such as the performance index are not ideal for capturing small changes in student outcomes and whether there are important subgroups of students that benefit. More detailed achievement data is needed to more confidently determine whether increased spending from passing levies affects student achievement.

We document a relatively large effect of increased resources on retaining students from going to digital charters, but this average effect likely masks important heterogeneity. Future work should explore the type of district that sees the largest benefit from increased spending and the composition of students that remain at a TPS that otherwise would have gone to an online charter school. Student-level data, particularly those with the ability to track students between schools, would be ideal for these analyses.

3.7 Figures & Tables

Table 3.1: Data Sources

Organization	Source	Relevant Variables	Years
Ohio Secretary of State	Election Results Database	Bond/Levy: millage; dollar value; purpose; vote share	1996-2012
Ohio Department of Education (ODE)	District Foundation Settlement Reports: Community School Deduction	# Students Transferred to Charter Schools	2002-2012
	ODE–Advanced Reports: District Enrollment	Total district enrollment	1995-2011
Ohio Department of Taxation	School District Property Tax Database	District-level property values	1986-2013
NCES Common Core of Data	School District Universe Survey	Special Education Enrollment; Number of Teachers Employed	1987-2011
	School Building Universe Survey	Student enrollment, by race; FRPL eligible student enrollment	1987-2011
	School District Finance Survey	Revenue and expenditures; Payments to charter schools	1989-2011

Notes: NCES=National Center for Education Statistics. FRPL=Free/Reduced-price lunch. FRPL eligible student enrollment is unavailable in 2007.

Table 3.2: Summary Statistics for Ohio School Property Tax Levy Elections

	(1)	(2)	(3)	(4)	(5)	(6)
		Capital Levies			General Purpose Levies	
Year	Number of Measures	Avg. Amount per Pupil	Fraction Approved	Number of Measures	Avg. Amount per Pupil	Fraction Approved
1996	126	807	0.46	98	221	0.60
1997	194	647	0.56	176	120	0.66
1998	197	778	0.51	161	163	0.66
1999	217	629	0.65	163	157	0.66
2000	197	1,259	0.69	184	247	0.76
2001	146	1,184	0.63	154	197	0.68
2002	151	2,216	0.57	171	197	0.63
2003	154	1,629	0.51	211	243	0.57
2004	153	1,038	0.55	336	259	0.46
2005	132	1,320	0.55	284	245	0.55
2006	130	1,073	0.61	207	303	0.52
2007	143	988	0.52	190	289	0.56
2008	141	1,755	0.59	202	338	0.57
2009	97	1,165	0.58	206	309	0.64
2010	97	1,557	0.57	254	311	0.54
2011	78	1,003	0.58	220	300	0.54
2012	77	1,361	0.66	194	331	0.60

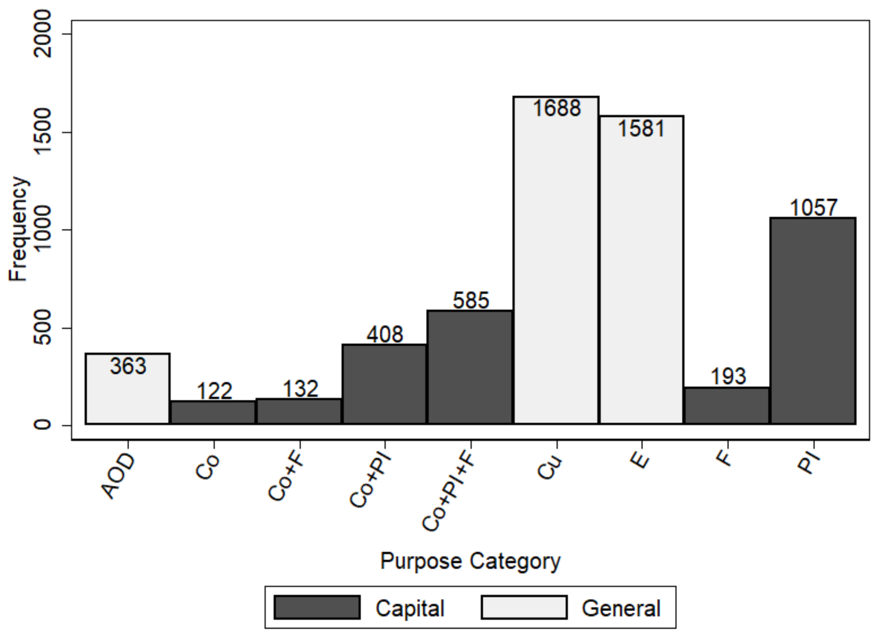
Notes: Capital levies include bond debt issued as well as levies for permanent improvements or facilities. General purpose levies are for current expenses, emergency requirements, and to avoid an operating deficit.

Table 3.3: District Summary Statistics

	(1) All	(2) Year Before Pass	(3) Year Before Fail	(4) Pass-Fail
	A. Per-Pupil Expenditures			
Capital	1,206	1,203	971	233
	(2,868)	(2,385)	(1,819)	(0.000)
Instructional	5,124	5,158	5,218	-60
	(1,747)	(869)	(1,635)	(0.119)
Support Services	3,225	3,260	3,251	9
	(2,809)	(812)	(1,645)	(0.819)
Total	10,355	10,422	10,240	182
	(6,335)	(3,208)	(4,097)	(0.087)
N	12,842	2,129	3,049	
	B. Fraction of Students Transferring			
Digital Charters	0.011	0.011	0.010	0.001
	(0.011)	(0.010)	(0.011)	(0.007)
Brick-and-Mortar Charters	0.008	0.008	0.007	0.001
	(0.025)	(0.026)	(0.024)	(0.245)
Any Charter	0.017	0.018	0.016	0.002
	(0.028)	(0.028)	(0.027)	(0.078)
N	7,345	1,429	1,800	
	C. Student and Teacher Counts			
# Students	2,893	3,067	3,012	55
	(4,893)	(4,117)	(4,515)	(0.655)
# Teachers	760	466	503	-37
	(2,144)	(1,062)	(1,495)	(0.326)
N	16,515	2,146	3,060	

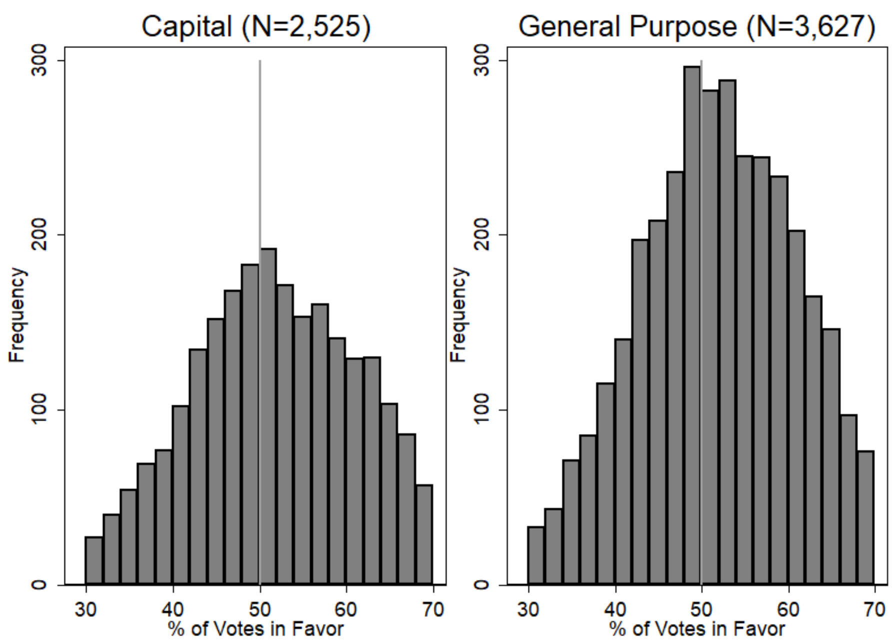
Notes: Columns (1)-(3) are means with standard deviations in parentheses. Column (4) shows the difference between columns (2) and (3) with a p-value for the difference in parentheses.

Figure 3.1: Levy Purpose Categories



Notes: AOD = Avoid an Operating Deficit, Co = Construction, F = Facilities, PI = Permanent Improvements, Cu = Current Expenditures, E = Emergency. Combinations not shown include Co + Cu (2), Co + F + Cu (1), Co + PI + Cu (31), Co + PI + F + Cu (11), Cu + AOD (3), Cu + E (2), E + AOD (1), F + Cu (2), PI + AOD (1), PI + Cu (46), and PI + F (30).

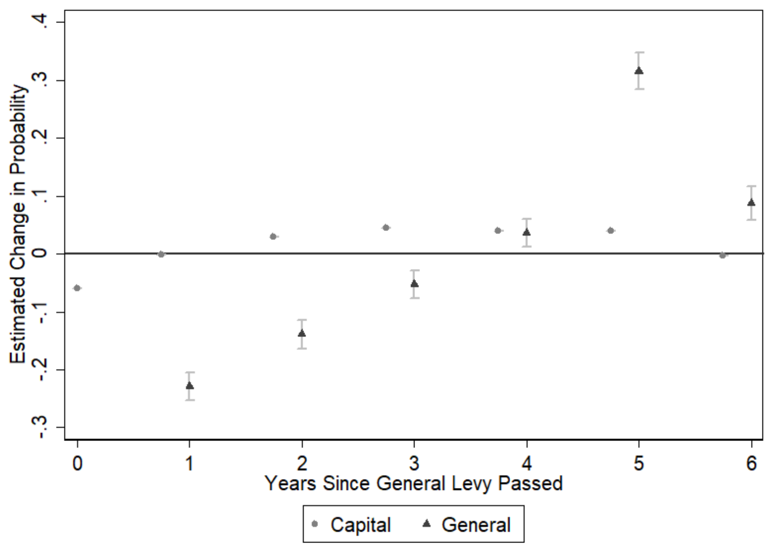
Figure 3.2: Density of Vote Share for Capital and General Purpose Levy Elections



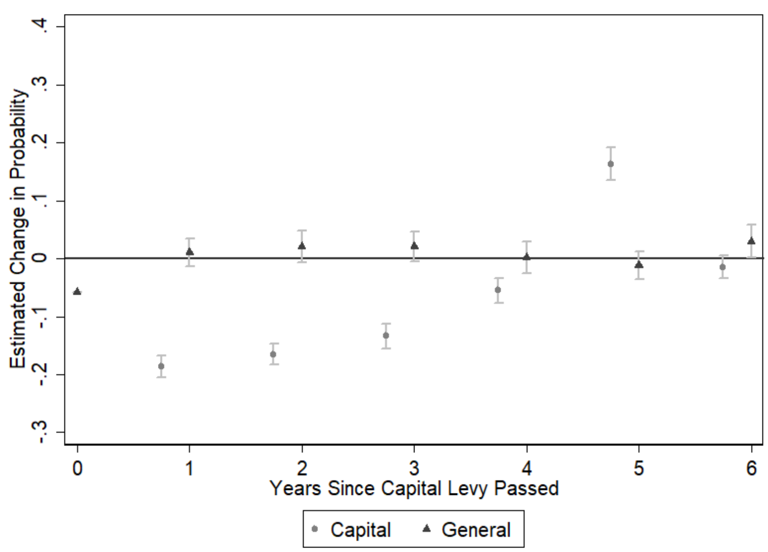
Notes: Sample includes all elections in Ohio school districts from 1996 to 2012. Vote shares are censored at 30 and 70.

Figure 3.3: Likelihood of Passing an Additional Levy After Levy Passage

A. After General Levy is Passed



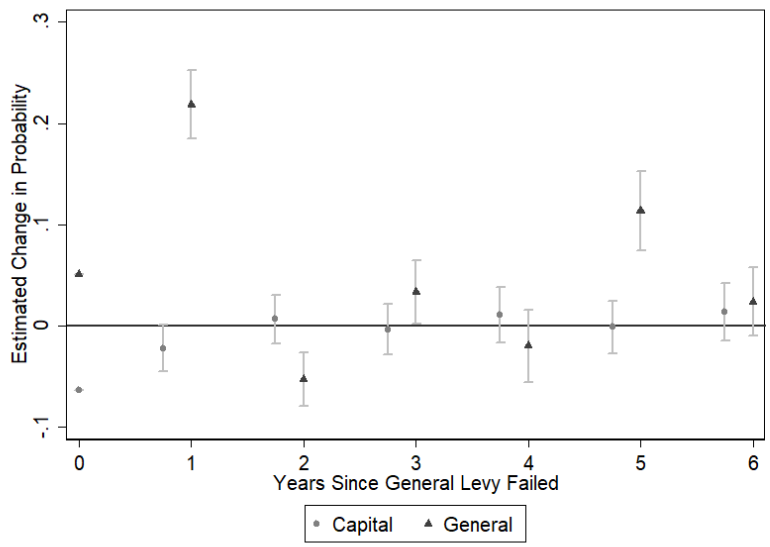
B. After Capital Levy is Passed



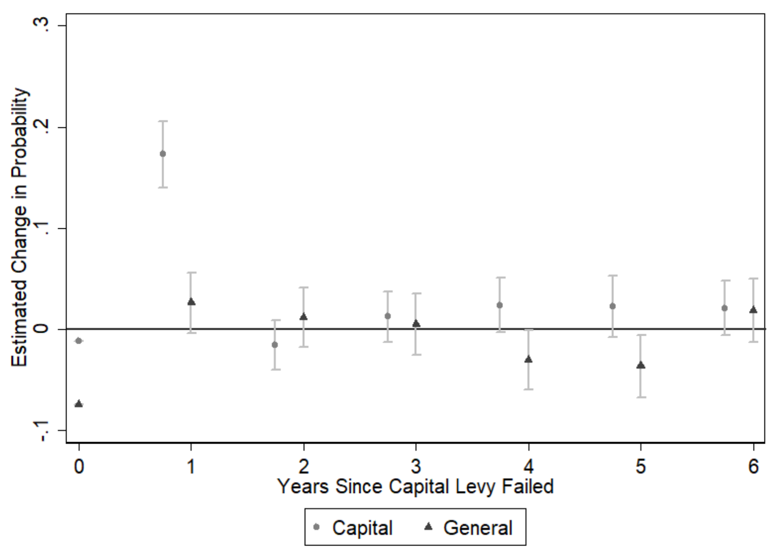
Notes: Coefficients are plotted from a regression with year and district fixed effects and standard errors clustered at the district level with 95 percent confidence intervals.

Figure 3.4: Likelihood of Passing an Additional Levy After Levy Failure

A. After General Levy is Defeated

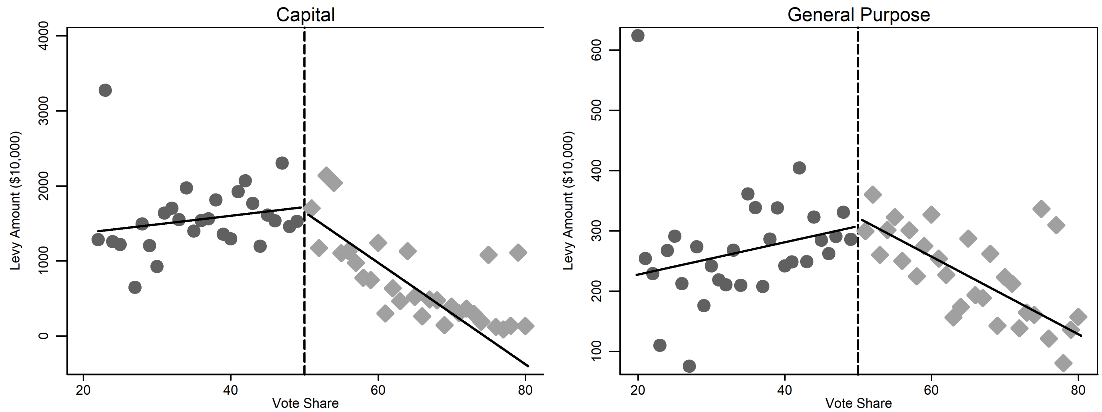


B. After Capital Levy is Defeated



Notes: Coefficients are plotted from a regression with year and district fixed effects and standard errors clustered at the district level with 95 percent confidence intervals.

Figure 3.5: Levy Amounts by Vote Share



Notes: Levy amounts are based on the stated amount of the capital project or the proposed millage rate times the taxable value of property in that year. Vote shares are censored at 20 and 80.

Figure 3.6: McCrary Tests for Capital and General Purpose Levy Elections

A. Capital Levies B. General Purpose Levies

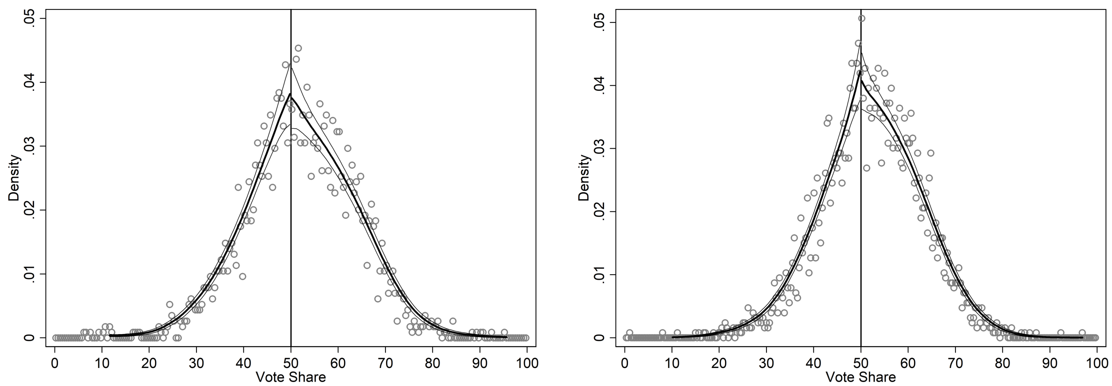


Table 3.4: McCrary Density Test Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	Levy Purpose		Election Month				Levy Length		
		Capital	General	Aug	Nov	Feb	Mar/May	CPT	5 Years	8 Years
$\hat{\theta}$	-0.051	-0.021	-0.045	-0.138	-0.043	0.195	-0.086	-0.062	-0.055	0.009
	(0.065)	(0.094)	(0.081)	(0.183)	(0.093)	(0.301)	(0.095)	(0.131)	(0.097)	(0.193)
N	18,832	2,525	3,627	476	3,144	235	2,401	1,120	3,125	427

Notes: Estimates for the discontinuity in the density of vote shares at the 50 percent threshold ($\hat{\theta}$) are shown with standard errors in parentheses.

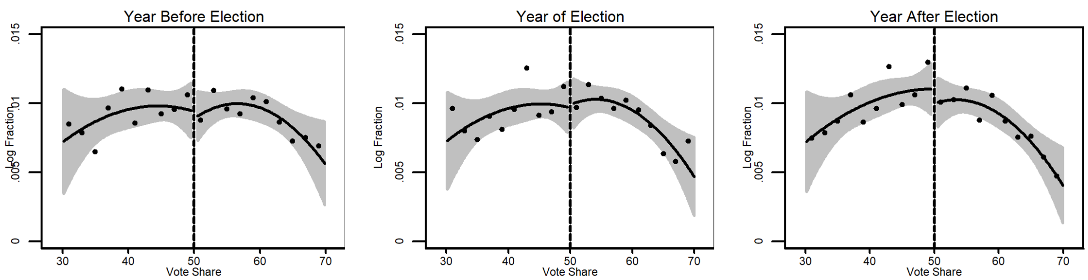
Table 3.5: Balance of Pre-election Covariates

	(1)	(2)	(3)	(4)
	Year before election $t-1$		Change from $t-2$ to $t-1$	
	Capital	General	Capital	General
A. District Outcomes				
Fraction Black Students	0.008	0.008	0.001+	0.001+
	(0.010)	(0.010)	(0.001)	(0.001)
Fraction FRL Students	0.004	0.004	-0.001	-0.001
	(0.010)	(0.010)	(0.004)	(0.004)
% AYP Indicators Met	-0.013	-0.013	-0.003	-0.003
	(0.017)	(0.017)	(0.008)	(0.008)
B. Finance Outcomes				
Total Expenditures (in thousands)	3,658	3,658	556	556
	(3,050)	(3,050)	(436)	(436)
Capital Outlays (in thousands)	483	483	-48	-48
	(499)	(499)	(321)	(321)
Instructional (in thousands)	2,053	2,053	76	76
	(1462)	(1,462)	(134)	(134)
Total Property Value (in thousands)	45167	45167	1,807	1,807
	(37444)	(37444)	(2,354)	(2,354)
C. Charter Outcomes				
Fraction of Any Charter Transfers (× 100)	-0.289	-0.289	-0.012	-0.012
	(0.218)	(0.218)	(0.072)	(0.072)
Fraction of Digital Charter Transfers (× 100)	-0.031	-0.031	-0.014	-0.014
	(0.081)	(0.081)	(0.040)	(0.040)
Fraction of B&M Charter Transfers (× 100)	-0.259	-0.259	0.053	0.053
	(0.194)	(0.194)	(0.075)	(0.075)

Notes: Estimates and standard errors (in parentheses) are presented in each cell for the effect of passing either a capital or general purpose levy on the given outcome during the year before for vote passed. Columns (1) and (2) present results on $t-1$ levels, while Columns (3) and (4) present results on $t-2$ to $t-1$ changes in outcomes. This setup follows after Table III in Cellini et al. (2010). + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Figure 3.7: Fraction of Students Lost to Digital Charters by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

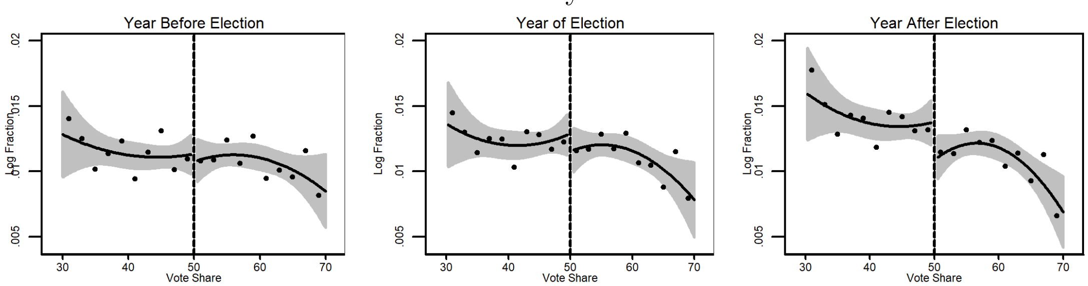
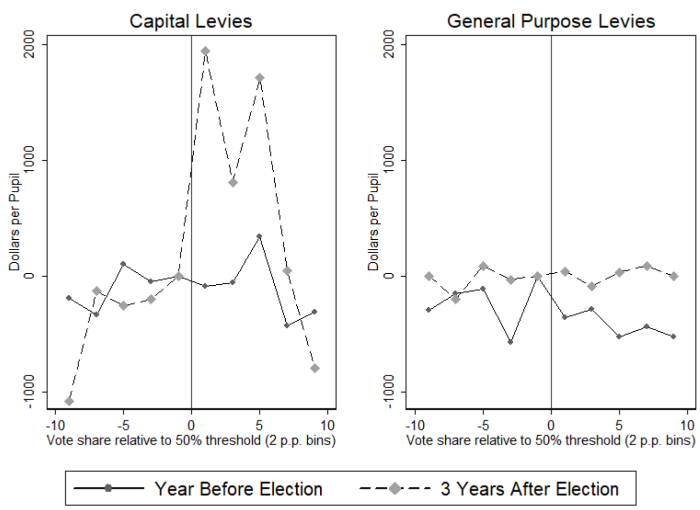
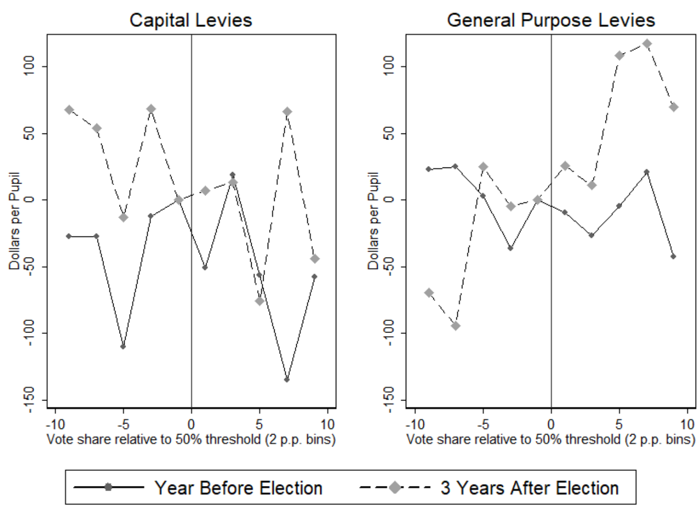


Figure 3.8: Spending, by Vote Share, Before and After Election

A. Capital Spending Per Pupil



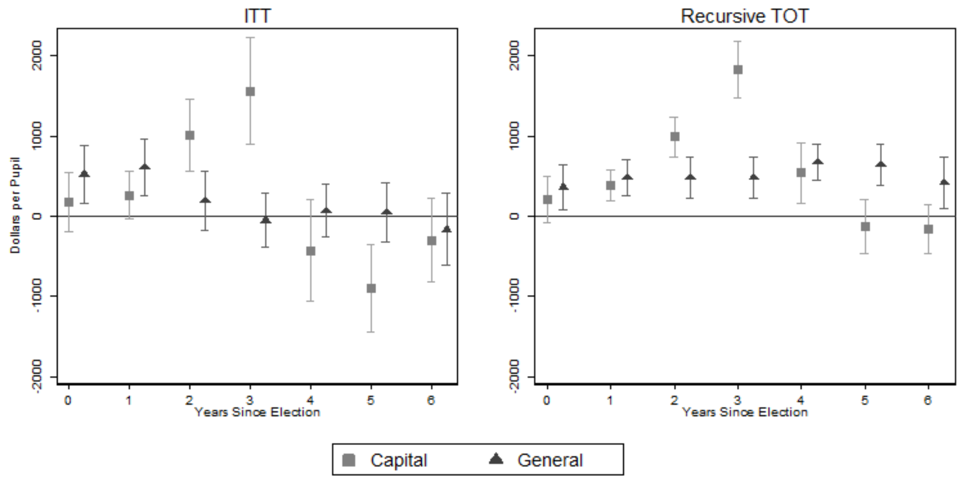
B. Instructional Spending Per Pupil



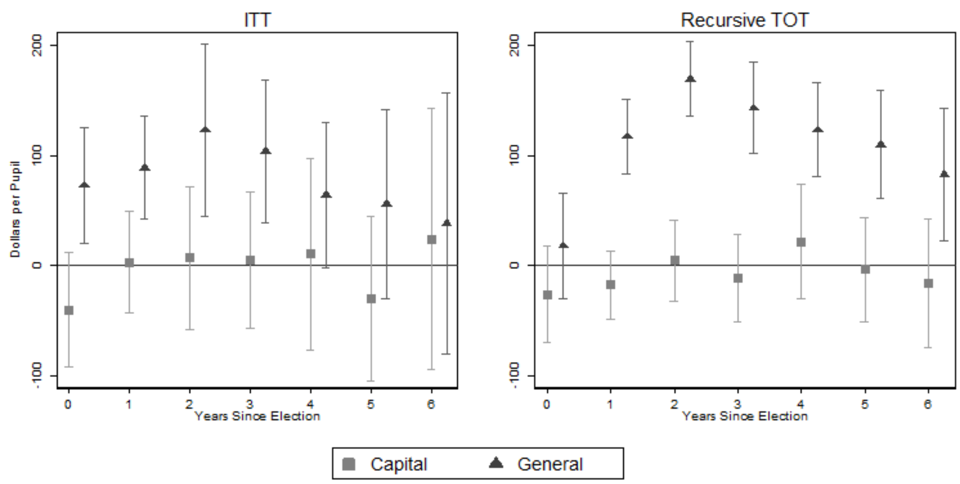
Notes: Graphs show spending per pupil for capital levies (left panels) and general purpose levies (right panels), by vote share in the focal election. Elections are grouped into bins of 2 percentage points. Averages control for year effects and are normalized to the first bin to the left of the 50% threshold.

Figure 3.9: Dynamic Effect of Levy Passage on Spending

A. Capital Spending Per Pupil



B. Instructional Spending Per Pupil



Notes: Graph shows the “intent-to-treat” and “recursive treatment-on-the-treated” effects and 95 percent confidence intervals of each relative year after levy passage on the capital spending per pupil in panel A and instruction spending per pupil in panel B. The specifications are outlined in equations 3.1 and 3.2. Corresponding regression output is provided in Table C.5.

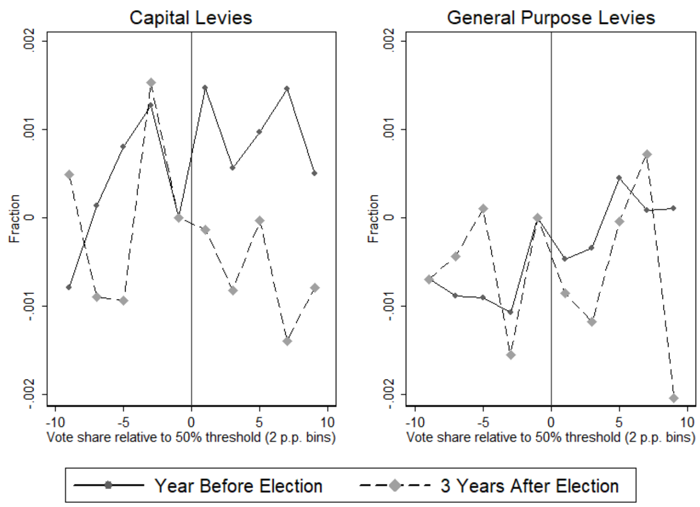
Table 3.6: Total Effect of Levy Passage on Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Capital Levies				General Levies		
	Total	Capital	Inst.	Support	Total	Capital	Inst.	Support
				A. ITT Estimates				
Levy Passed	2,101+	1,393	-19	189	2,067*	1,232	548**	406*
	(1,182)	(1,051)	(184)	(163)	(1,047)	(872)	(188)	(175)
				B. Recursive TOT Estimates				
Levy Passed	4,013**	3,668**	-47	-85	4,535**	3,553**	766**	232*
	(772)	(700)	(122)	(106)	(804)	(709)	(122)	(107)
Dep. Var. Mean	10,658	1,466	5,183	3,201	10,904	1,149	5,412	3,434
N	20,714	20,714	20,714	20,714	28,123	28,123	28,123	28,123

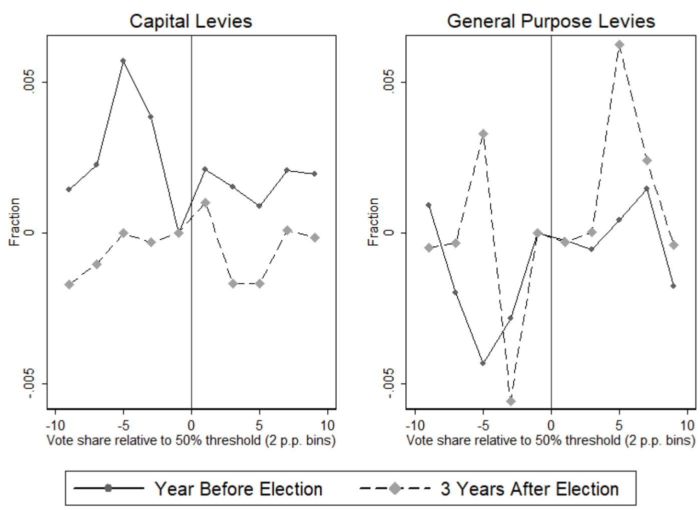
Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Figure 3.10: Students Lost to Charter Schools, by Vote Share, Before and After Election

A. Fraction of Students Lost to Digital Charters



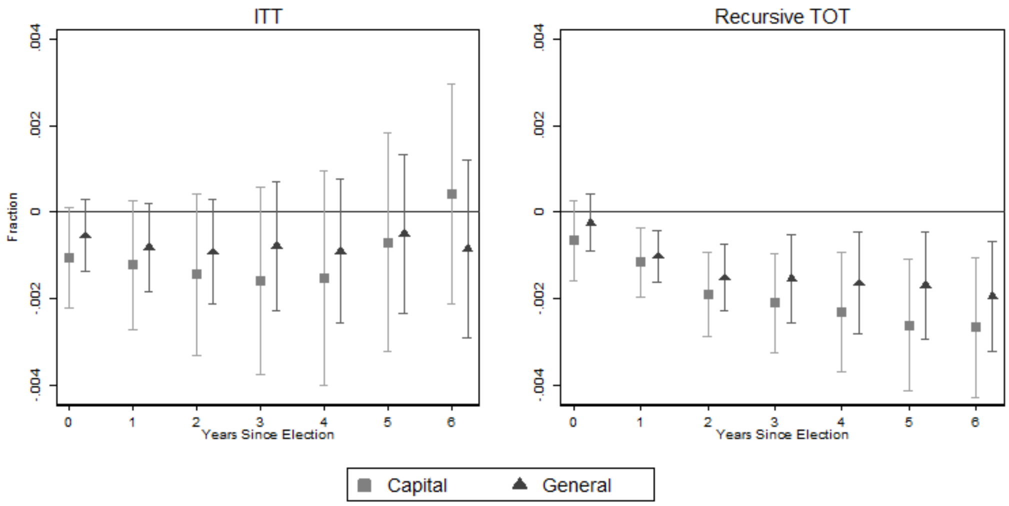
B. Fraction of Students Lost to Brick-and-Mortar Charters



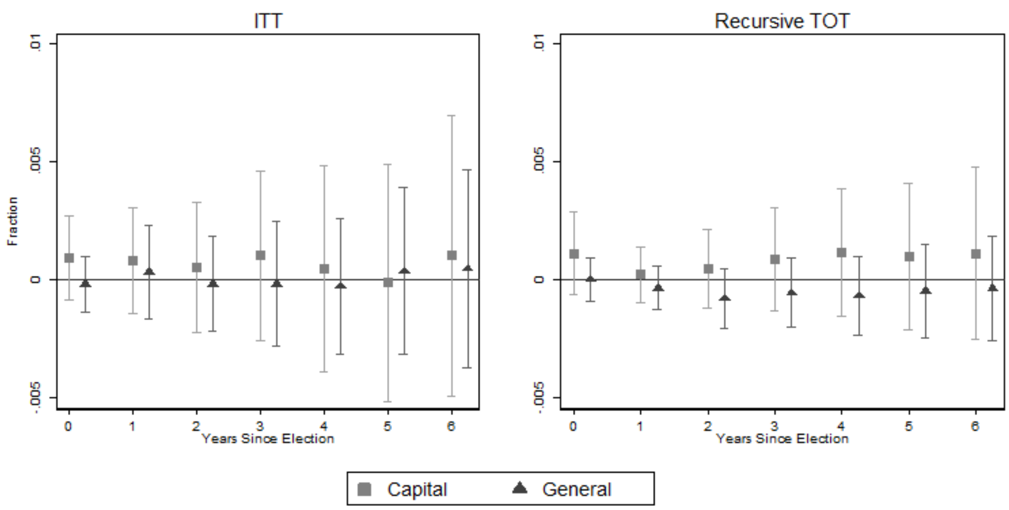
Notes: Graphs show the fraction of students lost to charter schools for capital levies (left panels) and general purpose levies (right panels), by vote share in the focal election. Elections are grouped into bins of 2 percentage points. Averages control for year effects and are normalized to the first bin to the left of the 50% threshold.

Figure 3.11: Effect of Levy Passage on Fraction of Students Lost to Charter Schools

A. Digital Charters



B. Brick-and-Mortar Charters



Notes: Graph shows the "intent-to-treat" and "recursive treatment-on-the-treated" estimates and 95 percent confidence intervals of each lag of levy passage on the fraction of students lost to charter schools. The specifications are outlined in equations 3.1 and 3.2, respectively. Corresponding regression output is provided in Table C.6.

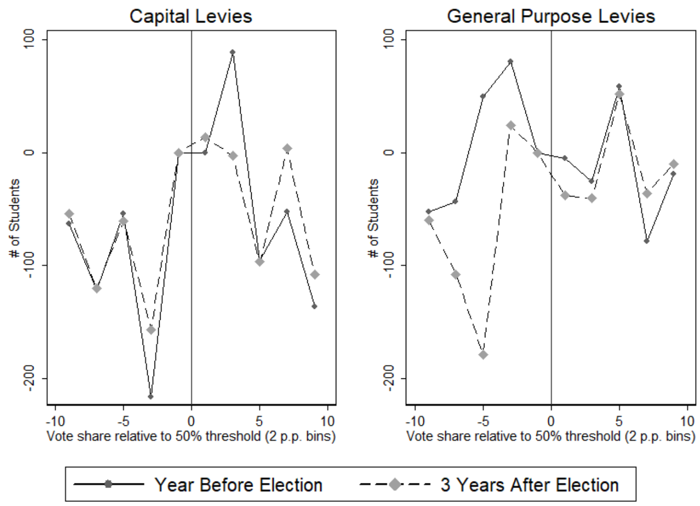
Table 3.7: Total Effect of Levy Passage on Fraction of Students Lost to Charter Schools

	(1)	(2)	(3)	(4)
	Capital Levies		General Levies	
	Digital	Brick and Mortar	Digital	Brick and Mortar
		A. ITT Estimates		
Levy Passed	-0.007	0.005	-0.005	0.0004
	(0.006)	(0.012)	(0.004)	(0.008)
		B. Recursive TOT Estimates		
Levy Passed	-0.013**	0.006	-0.010**	-0.003
	(0.004)	(0.008)	(0.003)	(0.005)
Dep. Var. Mean	0.010	0.006	0.012	0.010
N	15,168	15,168	22,381	22,381

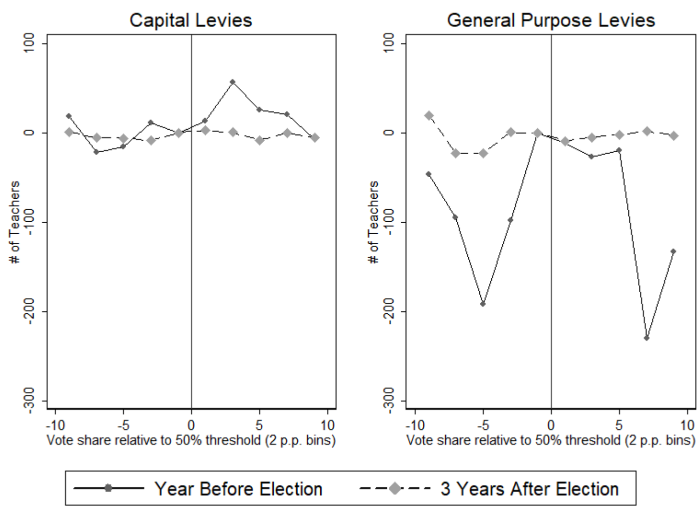
Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Figure 3.12: Student and Teacher Counts, by Vote Share, Before and After Election

A. Student Counts



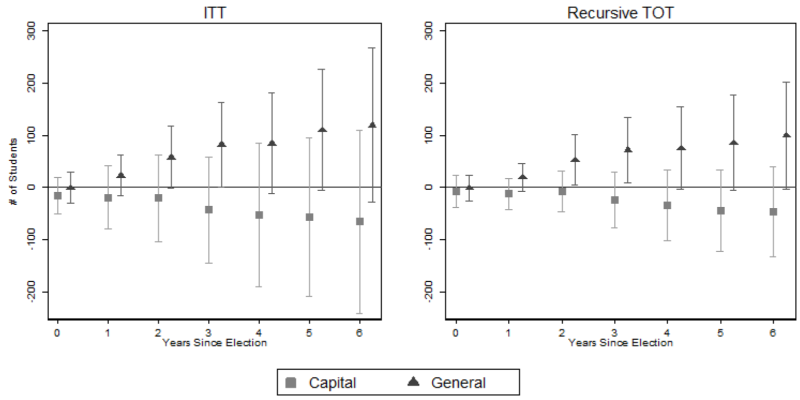
B. Teacher Counts



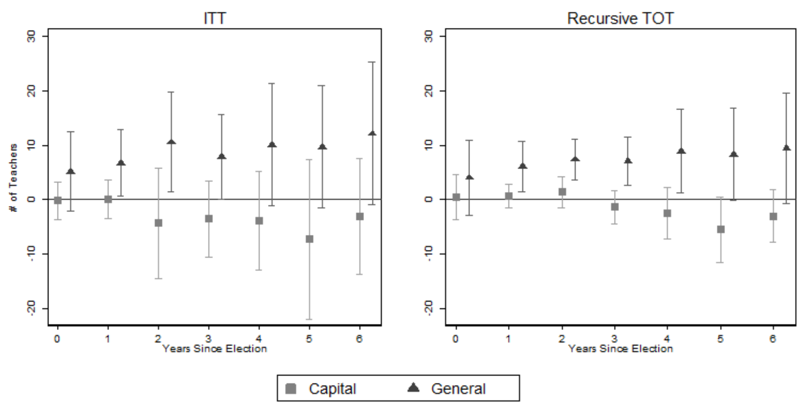
Notes: Graphs show student and teacher counts for capital levy (left panels) and general purpose levy (right panels) referenda, by vote share in the focal election. Elections are grouped into bins of 2 percentage points. Averages control for year effects and are normalized to the first bin to the left of the 50% threshold.

Figure 3.13: Student and Teacher Counts

A. Number of Students



B. Number of Teachers



C. Student-Teacher Ratio

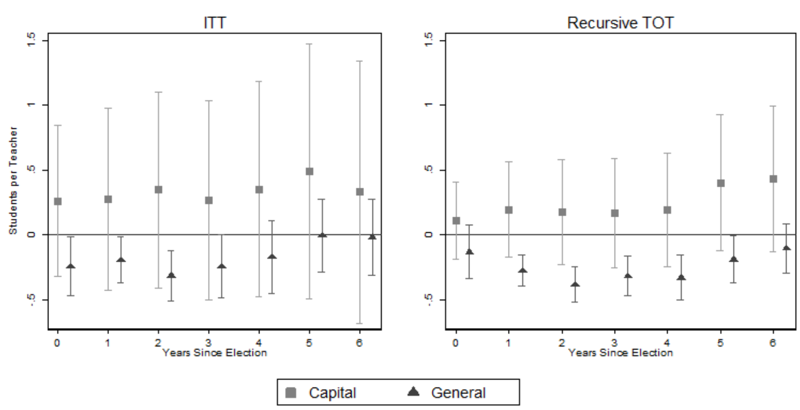


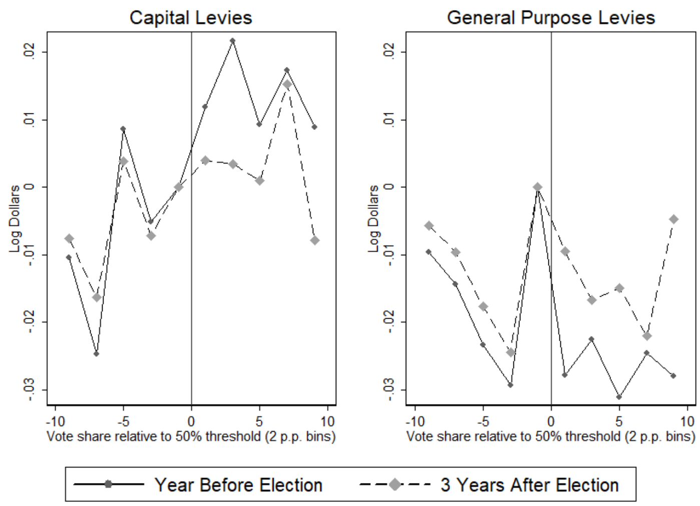
Table 3.8: Total Effect of Passing Levies on Teacher and Student Counts

	(1)	(2)	(3)	(4)	(5)	(6)
		Capital Levies			General Levies	
	# of Students	# of Teachers	Students / Teachers	# of Students	# of Teachers	Students / Teachers
			A. ITT Estimates			
Levy Passed	-272	-22	2.32	475+	62*	-1.19+
	(370)	(26)	(2.78)	(274)	(31)	(0.64)
			B. Recursive TOT Estimates			
Levy Passed	-176	-10	1.68	403+	51*	-1.74**
	(192)	(10)	(1.48)	(215)	(22)	(0.47)
Dep. Var. Mean	2,699	156	17.69	3,226	188	17.52
N	20,848	20,848	20,828	28,280	28,280	28,275

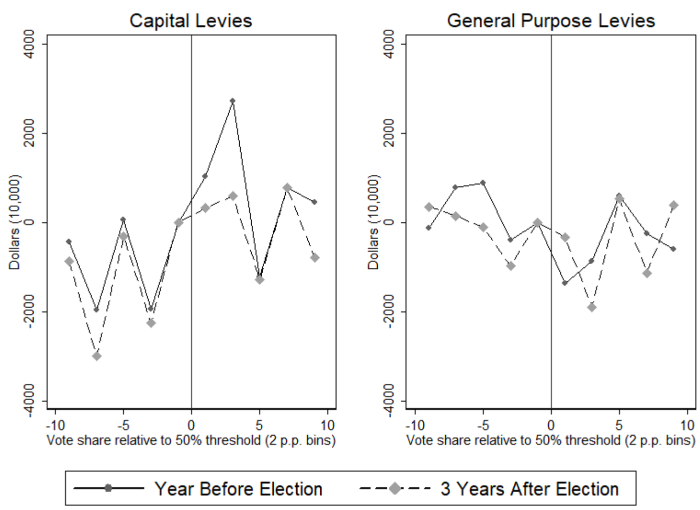
Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Figure 3.14: House Prices, by Vote Share, Before and After Election

A. Log Residential Home Prices



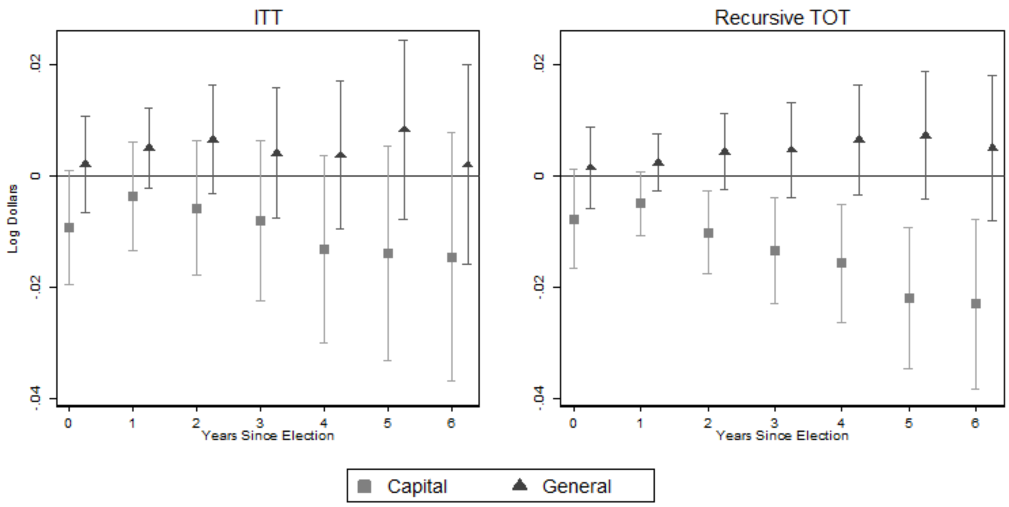
B. Residential Home Prices ($10,000s)



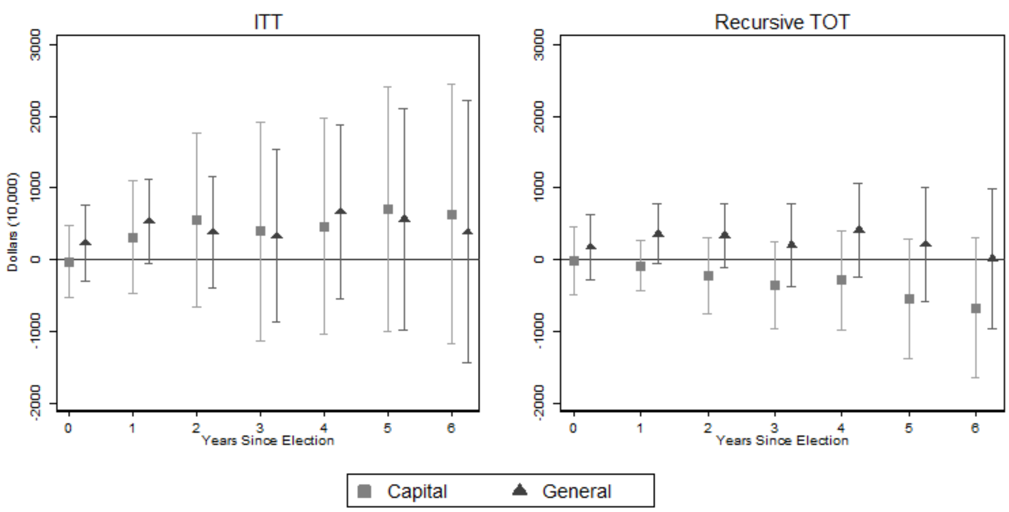
Notes: Graphs show residential home prices for capital levy (left panels) and general purpose levy (right panels) referenda, by vote share in the focal election. Elections are grouped into bins of 2 percentage points. Averages control for year effects and are normalized to the first bin to the left of the 50% threshold.

Figure 3.15: Effect of Levy Passage on Residential Values

A. Log Residential Home Values



B. Residential Home Values ($10,000)



Notes: Graph shows the "intent-to-treat" and "recursive treatment-on-the-treated" estimates and 95 percent confidence intervals of each lag of levy passage on residential values in real 2011 dollars per pupil. The specifications are outlined in equations 3.1 and 3.2, respectively. Corresponding regression output is provided in Table C.9.

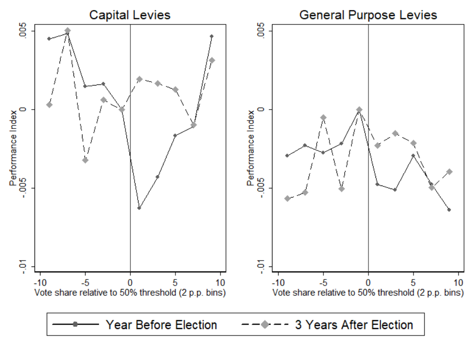
Table 3.9: Total Effect of Levy Passage on Residential Values

	(1)	(2)	(3)	(4)
	Capital Levies		General Levies	
	\$10,000	Log	\$10,000	Log
	A. ITT Estimates			
Levy Passed	3,016	-0.069	3,113	0.032
	(4,090)	(0.045)	(3,514)	(0.036)
	B. Recursive TOT Estimates			
Levy Passed	-2,186	-0.097**	1,724	0.031
	(2,013)	(0.032)	(1,903)	(0.029)
Dep. Var. Mean	26,189	18.84	33,613	19.12
N	19,268	19,268	25,163	25,163

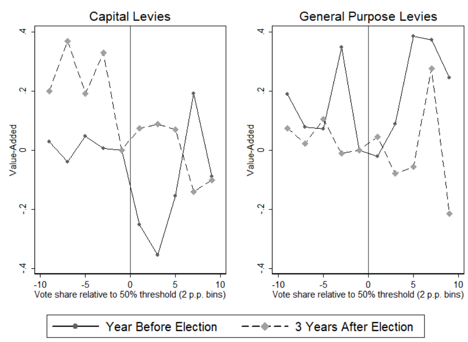
Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Figure 3.16: Student Outcomes, by Vote Share, Before and After Election

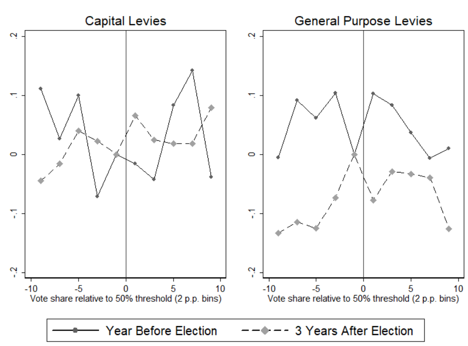
A. Performance Index



B. Value Added



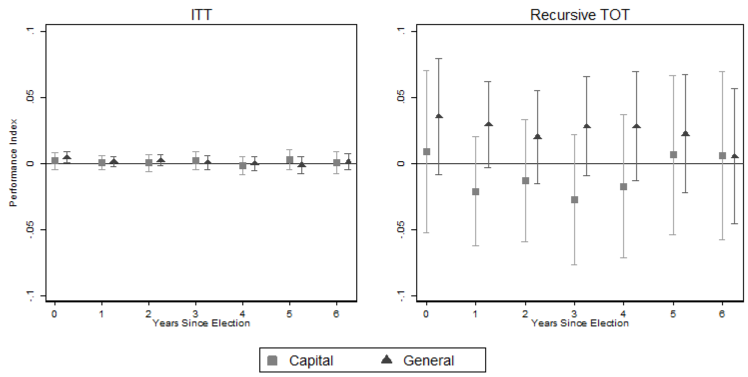
C. Fraction Meeting AYP



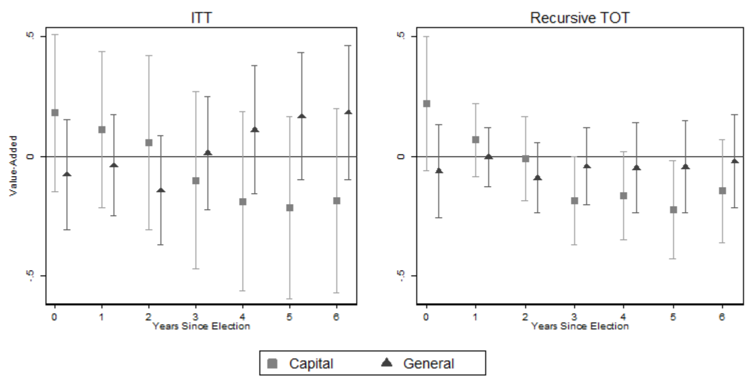
Notes: Graphs show student outcomes for capital levy (left) and general purpose levy (right) referenda, by vote share in the focal election. Elections are grouped into bins of 2 percentage points. Averages control for year effects and are normalized to the first bin to the left of the 50% threshold.

Figure 3.17: District Student Outcomes

A. Performance Index



B. Value Added



C. Fraction Meeting AYP

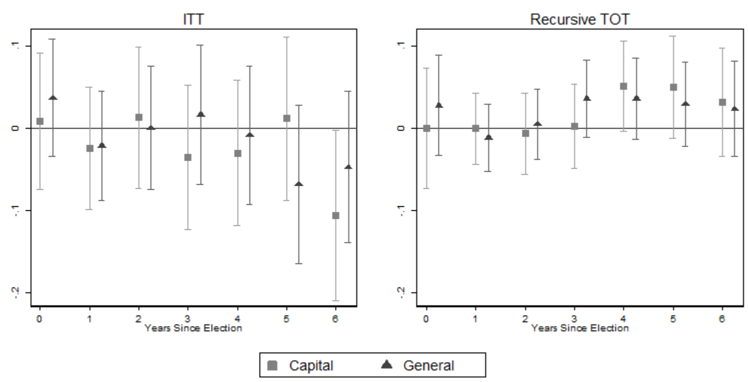


Table 3.10: Average Effect of Passing Levies on Student Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Capital Levies			General Levies		
	Performance Index	Value Added	AYP Met	Performance Index	Value Added	AYP Met
			A. ITT Estimates			
Levy Passed	0.006	-0.333	-0.163	0.009	0.225	-0.092
	(0.019)	(0.940)	(0.213)	(0.014)	(0.641)	(0.203)
			B. Recursive TOT Estimates			
Levy Passed	-0.058	-0.435	0.127	0.170	-0.304	0.145
	(0.154)	(0.537)	(0.146)	(0.122)	(0.486)	(0.126)
Dep. Var. Mean	-0.08	1.10	0.33	0.05	1.08	0.36
N	15,520	7,795	17,537	21,852	13,772	23,623

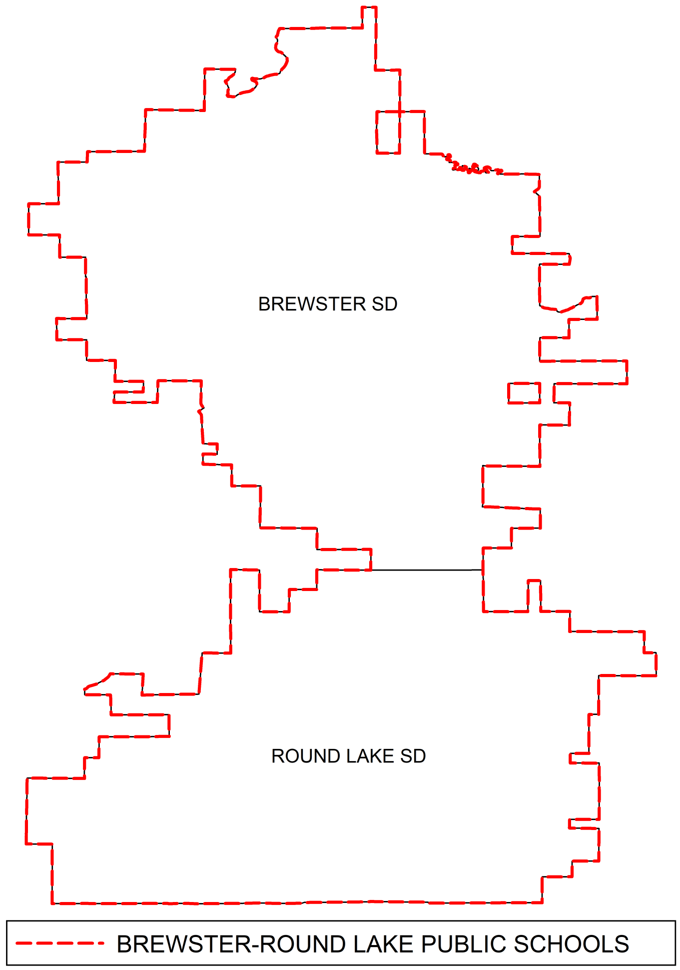
Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

APPENDIX A

THE EFFECT OF EDUCATION SPENDING ON STUDENT ACHIEVEMENT: EVIDENCE FROM PROPERTY VALUES AND SCHOOL FINANCE RULES – APPENDIX

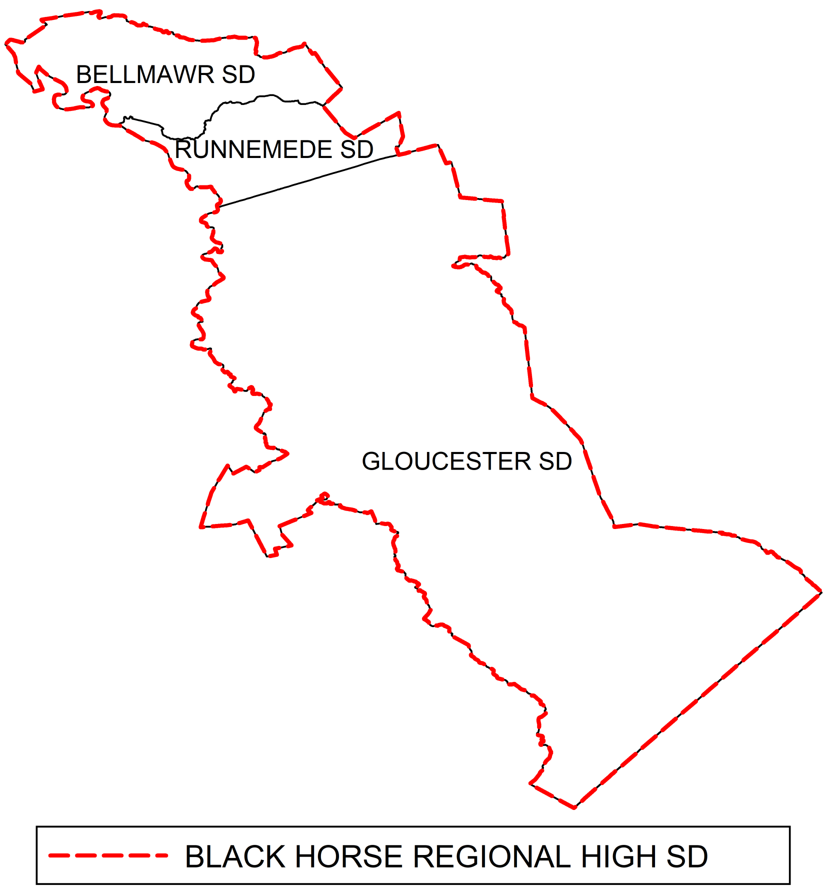
A.1 Online Appendix - Additional Figures and Tables

Figure A.1: Example of District Consolidation - Minnesota



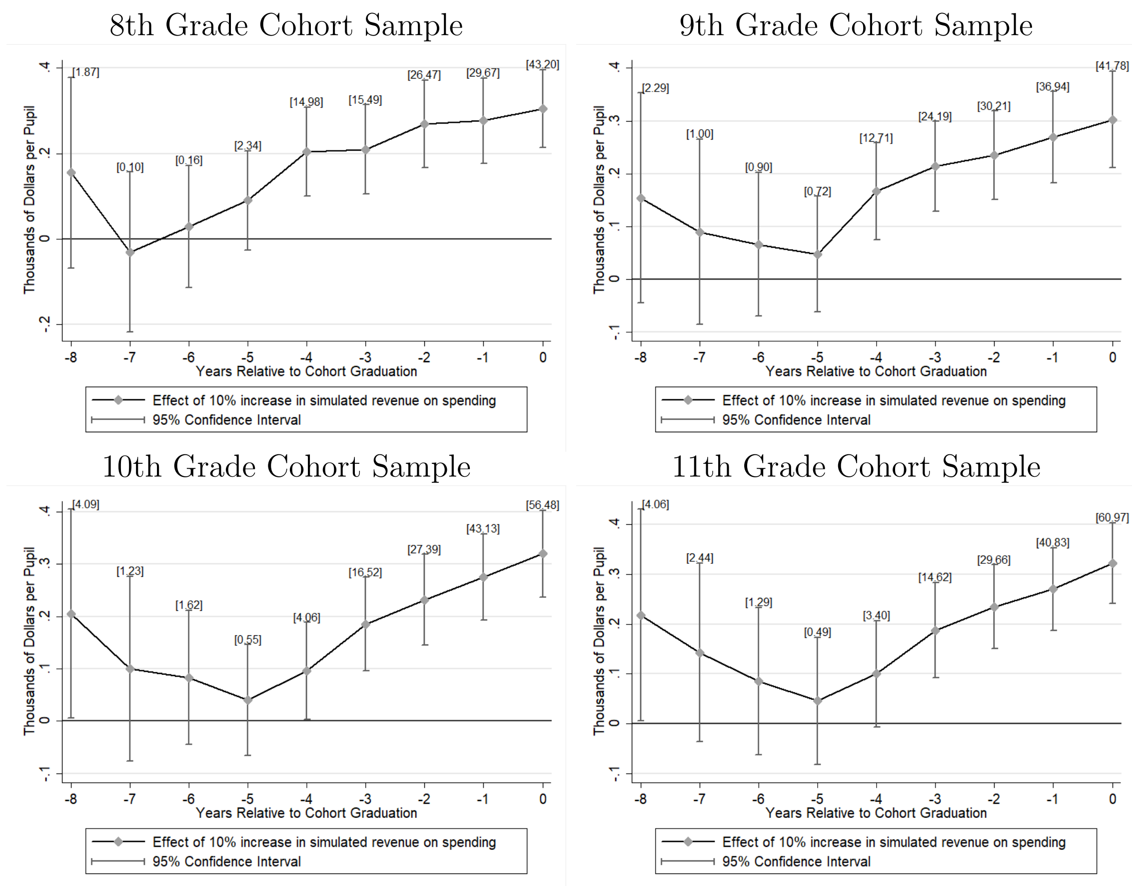
Notes: Boundaries shown for 2 school districts (Brewster and Round Lake) in Minnesota, which consolidated into a single school district (Brewster-Round Lake) in 2014.

Figure A.2: Example of Overlapping/Nested Districts - New Jersey



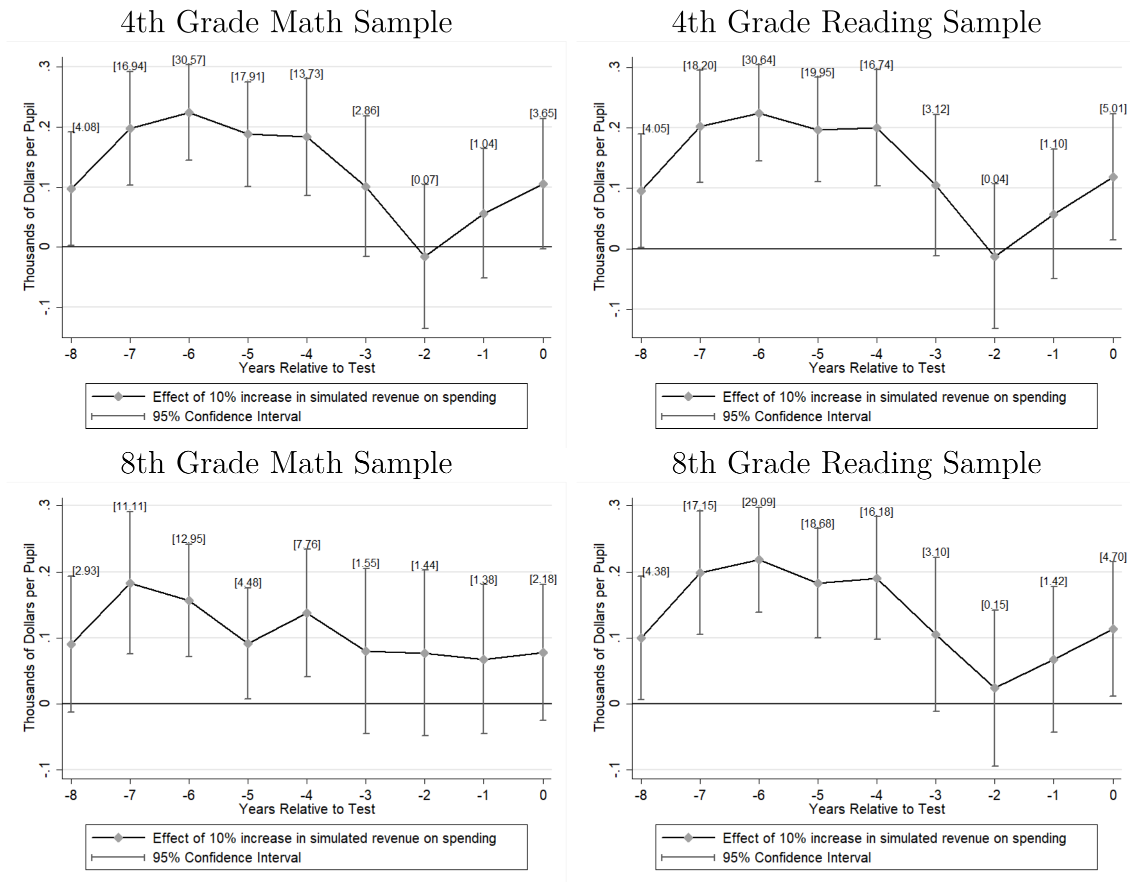
Notes: Boundaries for 4 school districts in New Jersey are shown. Bellmawr, Runnemede, and Gloucester are K-8 districts and Black Horse is a regional 9-12 district.

Figure A.3: First-stage effect of simulated revenue ($1,000 per pupil) on total expenditure ($1,000 per pupil) in graduation rate samples – individual year lags



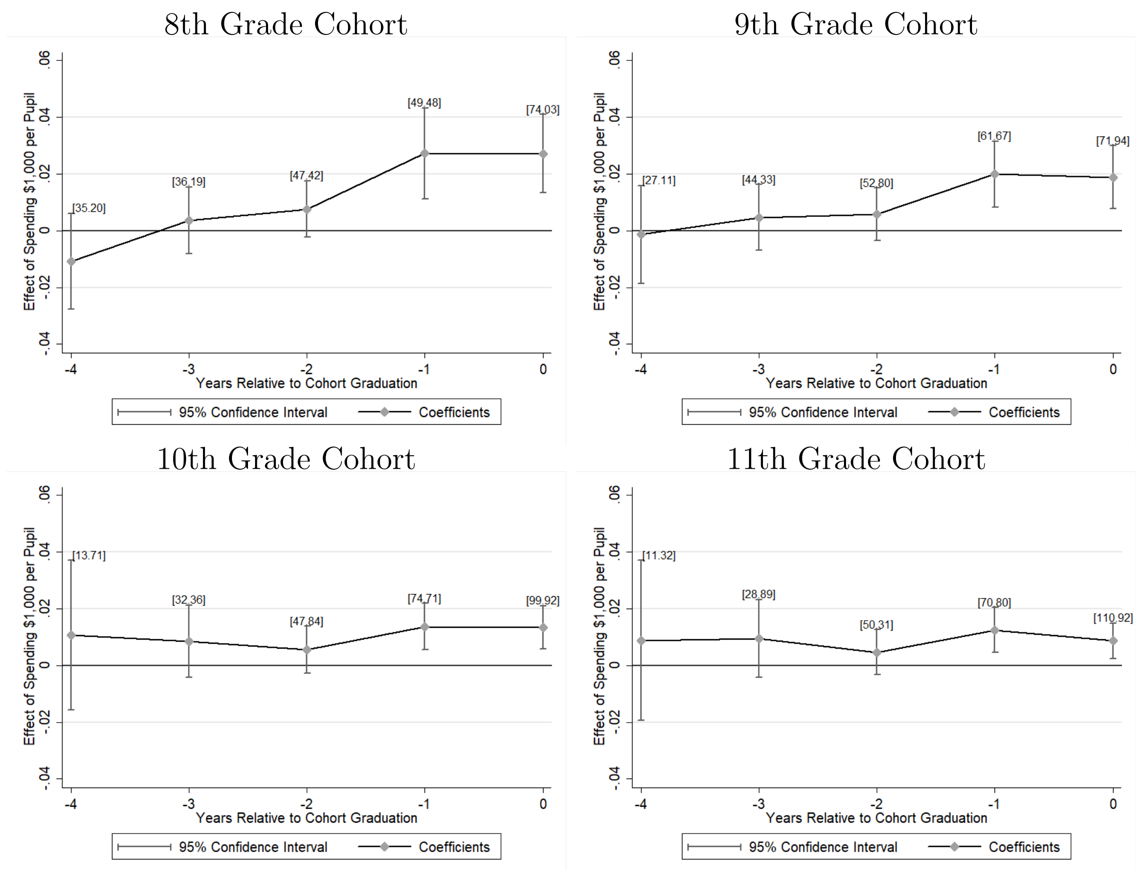
Notes: Figure A.3 presents point estimates, 95% confidence intervals, and F statistics in brackets, from individual regressions of lagged per-pupil total expenditures on log simulated revenue with the same lag for samples with non-missing graduation rates. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level. Corresponding estimates are presented in Table A.8.

Figure A.4: First-stage effect of simulated revenue ($1,000 per pupil) on total expenditure ($1,000 per pupil) for SEDA test score samples – individual year lags



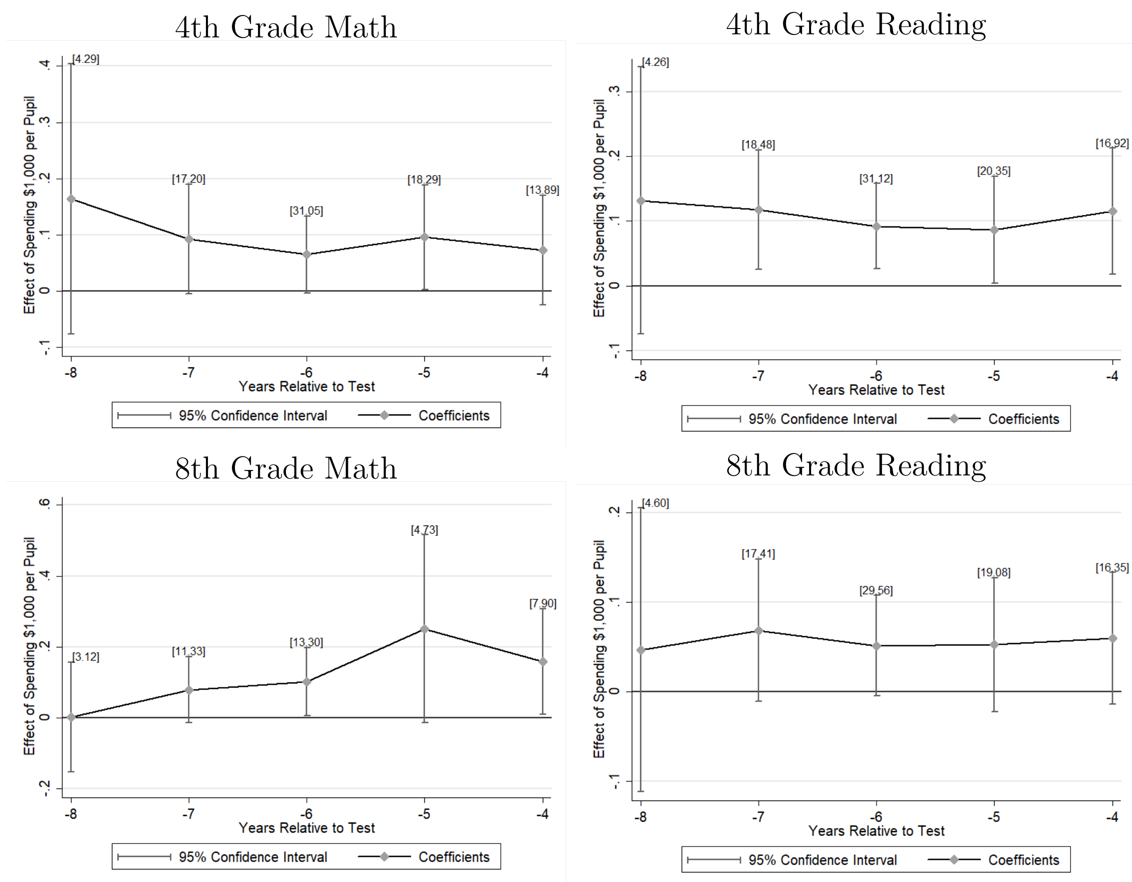
Notes: Figure A.4 presents point estimates, 95% confidence intervals, and F statistics in brackets, from individual regressions of lagged per-pupil total expenditures on log simulated revenue with the same lag for samples with non-missing graduation rates. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level. Corresponding estimates are presented in Table A.9.

Figure A.5: Two-stage least squares estimates of per-pupil spending on graduation rates



Notes: Figure A.5 presents 2SLS estimates and 95% confidence intervals for the effect of per-pupil spending instrumented with log simulated revenue. Numbers in brackets are first-stage F statistics. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level. Corresponding estimates are presented in Table A.11.

Figure A.6: Two-stage least squares estimates of per-pupil spending on SEDA test scores



Notes: Figure A.6 presents 2SLS estimates and 95% confidence intervals for the effect of per-pupil spending instrumented with log simulated revenue. Numbers in brackets are first-stage F statistics. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level. Corresponding estimates are presented in Table A.13.

Table A.1: States with Tax and Expenditure Limits

State	Description
Arkansas	Income limited to 5% for homesteads and 10% for non-homesteads
California	Increase in assessed value limited to $\min\{0.02, CPI\}$
Illinois	29 of 102 counties opted into the PTELL program by 1999. This limits the increase in property tax revenue to $\min\{0.05, CPI\}$
Indiana	The maximum levy is the maximum levy from the previous year adjusted by the assessed value growth quotient (AVGQ)
Iowa	Increase in property values limited to 3% annually
Maryland	10% limit in annual increase in property values with a 3-year phase in for all increases
Massachusetts	Annual increase in property tax revenue limited to 2.5%
Michigan	Annual increase in property values limited to $\min\{0.05, CPI\}$
Nebraska	Annual increase in spending limited by an amount determined by the legislature
Nevada	Annual increase in property tax revenue limited to 6%
New Jersey	Annual increase in spending limited to $\min\{0.03, CPI\}$
New Mexico	Annual increase in property values limited to 3%
Ohio	Tax rates automatically adjust as assessments increase to keep revenue generated from a tax levy fixed. 3-year phase in of value increases
Oklahoma	Annual increase in property values limited to 5%
Oregon	Annual increase in property values limited to 3%
Texas	Annual increase in property values limited to 10%
Washington	Annual increase in revenue limited to 6% above the highest level in the last three years
West Virginia	Annual increase in revenue limited to 1% per year (tax rates decreased if assessments raise more than 1%)
Wisconsin	Annual increase in revenue per pupil cannot exceed $208.88 in 1998-1999, and adjusted for inflation in future years

States with no dynamic limits (as of FY1999): Alabama, Alaska, Arizona, Colorado. Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Kansas, Kentucky, Louisiana, Maine, Minnesota, Mississippi, Missouri, Montana, New Hampshire, New York, North Carolina, North Dakota, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, Vermont, Virginia, and Wyoming.

Table A.2: School Finance Formula Type for Each State

State	Foundation	District Power Equalization	Combination/ Tiered
Alabama	X		
Alaska	X		
Arizona	X		
Arkansas	X		
California	X		
Colorado	X		
Connecticut	X		
Delaware	X		
Florida	X		
Georgia			X
Idaho	X		
Illinois			X
Indiana	X		
Iowa	X		
Kansas	X		
Kentucky			X
Louisiana	X		
Maine	X		
Maryland	X		
Massachusetts	X		
Michigan	X		
Minnesota	X		
Mississippi	X		
Missouri	X		
Montana			X
Nebraska	X		
Nevada	X		
New Hampshire	X		
New Jersey	X		
New Mexico	X		
New York	X		
North Dakota	X		
Ohio	X		
Oklahoma	X		
Oregon	X		
Pennsylvania	X		
Rhode Island		X	
South Carolina	X		
South Dakota	X		
Tennessee	X		
Texas			X
Utah	X		
Vermont		X	
Virginia	X		
Washington	X		
West Virginia	X		
Wisconsin		X	
Wyoming	X		
Total	40	3	5

Notes: Adapted from Verstegen and Jordan (2009). Not included: Hawaii and North Carolina. Hawaii's single school district is fully funded by the state. North Carolina uses a flat grant system.

Table A.3: State Summary Statistics

	(1)	(2)	(3)	(4)	(5)
		Balanced Panel School Districts			
States	Raw Districts	All	SEDA Test Scores	SEDA Test Score Gaps	CCD Cohort Graduation Rate
Arkansas	245	238	237	49	234
Connecticut	167	138	135	27	106
Florida	67	67	67	57	67
Georgia	180	180	178	131	177
Idaho	115	112	84	2	97
Illinois	871	848	667	87	425
Iowa	364	319	283	16	273
Kansas	297	284	207	16	236
Kentucky	174	173	164	25	165
Massachusetts	408	235	225	29	210
Minnesota	337	329	289	32	290
Mississippi	149	79	79	58	79
Nevada	17	17	15	2	16
New Hampshire	162	120	89	0	68
New Jersey	573	339	321	89	211
New Mexico	89	89	66	6	78
New York	725	668	618	89	592
North Carolina	115	115	115	84	115
North Dakota	192	176	54	2	104
Ohio	612	608	602	76	573
Oklahoma	539	513	322	21	357
Oregon	195	194	138	10	161
Texas	1,031	1,025	804	187	866
Washington	295	295	211	34	229
Total	7,919	7,161	5,970	1,129	5,729

Notes: The number of districts per state in my sample are shown. The first column reports the raw number of traditional public school districts reported in the CCD and column (2) is the number of districts in my balanced panel. Columns (3) through (5) are the number of districts with nonmissing values in the balanced panel for the variables indicated.

Table A.4: First stage estimates of log simulated revenue on log spending for graduation rate samples – individual year lags

	$\tau = 0$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$	$\tau = 8$
				A. 8th Grade Cohort Graduation Rate					
Log Sim. Rev.	0.142**	0.146**	0.151**	0.127**	0.130**	0.079*	0.033	-0.003	0.056
	(0.028)	(0.029)	(0.030)	(0.029)	(0.032)	(0.036)	(0.048)	(0.062)	(0.072)
F	25.83	24.38	25.62	18.66	16.23	4.76	0.48	0.00	0.60
Districts	2,724	2,724	2,722	2,721	2,720	2,716	2,708	2,688	2,668
N	17,467	17,467	17,275	16,967	15,677	14,640	13,203	11,510	9,147
				B. 9th Grade Cohort Graduation Rate					
Log Sim. Rev.	0.132**	0.141**	0.136**	0.153**	0.151**	0.092**	0.078+	0.051	0.092
	(0.024)	(0.024)	(0.024)	(0.025)	(0.029)	(0.034)	(0.044)	(0.054)	(0.063)
F	29.38	34.72	32.45	37.35	27.23	7.22	3.12	0.89	2.12
Districts	2,825	2,825	2,825	2,825	2,825	2,825	2,825	2,821	2,816
N	22,342	22,130	21,751	20,108	18,643	16,918	14,885	12,683	10,011
				C. 10th Grade Cohort Graduation Rate					
Log Sim. Rev.	0.144**	0.136**	0.135**	0.136**	0.105**	0.071*	0.067	0.058	0.110+
	(0.022)	(0.022)	(0.024)	(0.026)	(0.029)	(0.033)	(0.042)	(0.055)	(0.065)
F	42.80	37.50	31.22	28.26	13.42	4.46	2.52	1.11	2.88
Districts	2,825	2,825	2,825	2,825	2,825	2,825	2,825	2,822	2,817
N	23,457	23,082	21,468	19,863	18,063	16,449	14,593	12,633	9,969
				D. 11th Grade Cohort Graduation Rate					
Log Sim. Rev.	0.135**	0.138**	0.143**	0.142**	0.082**	0.058	0.062	0.081	0.109
	(0.021)	(0.023)	(0.024)	(0.028)	(0.033)	(0.038)	(0.047)	(0.057)	(0.068)
F	42.18	36.31	33.95	26.41	6.08	2.40	1.72	2.02	2.58
Districts	2,823	2,823	2,823	2,820	2,815	2,815	2,813	2,812	2,798
N	22,227	20,606	19,049	17,194	15,357	14,431	13,327	12,089	9,540

Notes: This table reports the results of individual first stage regressions of log total expenditures on log simulated revenue with the same lag for samples with non-missing graduation rates. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.5: First stage estimates of log simulated revenue on log spending for test score samples – individual year lags

	$\tau = 0$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$	$\tau = 8$
				A. SEDA 4th Grade Math Scores					
Log Sim. Rev.	0.072*	0.047+	0.021	0.074**	0.148**	0.197**	0.230**	0.265**	0.201**
	(0.034)	(0.029)	(0.035)	(0.031)	(0.026)	(0.025)	(0.022)	(0.028)	(0.030)
F	4.58	2.71	0.37	5.85	31.96	62.49	106.94	90.21	44.03
Districts	5,780	5,780	5,781	5,781	5,781	5,781	5,781	5,781	5,781
N	26,501	26,492	26,495	26,496	26,500	26,502	26,502	26,501	26,502
				B. SEDA 4th Grade Reading Scores					
Log Sim. Rev.	0.077*	0.049+	0.022	0.074**	0.153**	0.199**	0.230**	0.266**	0.201**
	(0.033)	(0.029)	(0.035)	(0.031)	(0.026)	(0.025)	(0.022)	(0.028)	(0.030)
F	5.34	2.86	0.41	5.89	34.34	64.14	106.22	91.05	44.14
Districts	5,778	5,778	5,779	5,779	5,779	5,779	5,779	5,779	5,779
N	26,511	26,502	26,505	26,506	26,510	26,512	26,512	26,511	26,512
				C. SEDA 8th Grade Math Scores					
Log Sim. Rev.	0.053	0.031	0.037	0.049+	0.107**	0.124**	0.161**	0.209**	0.133**
	(0.032)	(0.028)	(0.034)	(0.028)	(0.024)	(0.023)	(0.022)	(0.031)	(0.032)
F	2.65	1.20	1.19	3.17	20.39	28.63	52.43	46.60	17.64
Districts	5,787	5,787	5,787	5,788	5,788	5,788	5,788	5,788	5,788
N	24,741	24,732	24,734	24,736	24,740	24,742	24,741	24,740	24,741
				D. SEDA 8th Grade Reading Scores					
Log Sim. Rev.	0.076*	0.050+	0.035	0.070*	0.147**	0.191**	0.230**	0.270**	0.204**
	(0.033)	(0.029)	(0.034)	(0.030)	(0.026)	(0.024)	(0.022)	(0.028)	(0.030)
F	5.35	2.95	1.06	5.29	31.87	61.05	104.56	92.39	45.53
Districts	5,801	5,801	5,801	5,802	5,802	5,802	5,802	5,802	5,802
N	26,599	26,590	26,592	26,594	26,598	26,600	26,600	26,599	26,600

Notes: This table reports the results of individual first stage regressions of log total expenditures on log simulated revenue with the same lag for samples with non-missing test scores. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.6: Two-stage least squares estimates of log spending on graduation rates

	$\tau=0$	$\tau=1$	$\tau=2$	$\tau=3$	$\tau=4$	$\tau=5$	$\tau=6$	$\tau=7$	$\tau=8$
				A. 8th Grade Cohort Graduation Rate					
Log Spending	0.452**	0.414**	0.107	0.046	-0.134	0.001	-0.193	-0.318	-0.337
	(0.121)	(0.122)	(0.071)	(0.078)	(0.104)	(0.151)	(0.250)	(0.465)	(0.397)
Dep. Var. Mean	0.79	0.79	0.79	0.78	0.79	0.78	0.78	0.78	0.79
First-stage F	64.53	58.36	66.70	62.14	52.19	24.83	8.05	1.74	2.89
Districts	2,676	2,676	2,675	2,669	2,660	2,656	2,636	2,608	2,535
N	17,419	17,419	17,228	16,915	15,617	14,580	13,131	11,430	9,014
				B. 9th Grade Cohort Graduation Rate					
Log Spending	0.327**	0.304**	0.082	0.057	-0.014	-0.054	-0.284+	-0.475	-0.473
	(0.098)	(0.090)	(0.066)	(0.073)	(0.092)	(0.129)	(0.172)	(0.297)	(0.300)
Dep. Var. Mean	0.76	0.76	0.76	0.76	0.76	0.75	0.75	0.75	0.75
First-stage F	76.00	80.54	77.34	80.42	60.09	25.46	12.85	4.46	5.09
Districts	2,823	2,823	2,823	2,823	2,818	2,815	2,808	2,802	2,758
N	22,340	22,128	21,749	20,106	18,636	16,908	14,868	12,664	9,953
				C. 10th Grade Cohort Graduation Rate					
Log Spending	0.223**	0.215**	0.077	0.103	0.098	0.143	-0.121	-0.389	-0.201
	(0.064)	(0.064)	(0.060)	(0.078)	(0.122)	(0.158)	(0.196)	(0.261)	(0.202)
Dep. Var. Mean	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.80	0.81
First-stage F	109.75	93.67	73.56	65.35	40.78	19.81	11.11	4.80	6.35
Districts	2,824	2,824	2,824	2,824	2,822	2,822	2,818	2,804	2,773
N	23,456	23,081	21,467	19,862	18,060	16,446	14,586	12,615	9,925
				D. 11th Grade Cohort Graduation Rate					
Log Spending	0.147**	0.193**	0.064	0.112	0.091	0.102	-0.099	-0.411+	-0.360
	(0.054)	(0.062)	(0.056)	(0.080)	(0.146)	(0.178)	(0.209)	(0.229)	(0.239)
Dep. Var. Mean	0.85	0.85	0.85	0.85	0.85	0.84	0.85	0.86	0.86
First-stage F	119.16	90.27	75.76	61.81	26.19	13.51	8.77	6.33	5.77
Districts	2,820	2,819	2,815	2,810	2,803	2,794	2,786	2,773	2,722
N	22,224	20,602	19,041	17,184	15,345	14,410	13,300	12,050	9,464

Notes: This table reports results from individual two-stage least squares regressions of graduation rates on lagged log total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.7: Two-stage least squares estimates of log spending on SEDA test scores

	No Lag	1 Lag	2 Lags	3 Lags	4 Lags	5 Lags	6 Lags	7 Lags	8 Lags
					A. 4th Grade Math				
Log Spending	-2.474	-1.003	5.189	0.851	-2.474	0.910*	0.631*	0.690*	0.803
	(1.786)	(1.770)	(10.711)	(1.331)	(1.786)	(0.404)	(0.315)	(0.331)	(0.459)
First-stage F	6.91	4.94	0.36	6.15	6.91	64.22	108.80	91.49	45.09
Districts	5,663	5,662	5,662	5,663	5,663	5,664	5,664	5,664	5,664
N	26,384	26,374	26,376	26,378	26,384	26,385	26,385	26,384	26,385
					B. 4th Grade Reading				
Log Spending	-1.054	-2.209	6.814	1.139	-1.054	0.849*	0.899**	0.900**	0.641
	(1.362)	(1.832)	(12.045)	(1.321)	(1.362)	(0.377)	(0.291)	(0.301)	(0.408)
First-stage F	7.87	5.14	0.40	6.20	7.87	65.90	108.09	92.34	45.22
Districts	5,660	5,659	5,659	5,660	5,660	5,661	5,661	5,661	5,661
N	26,393	26,383	26,385	26,387	26,393	26,394	26,394	26,393	26,394
					C. 8th Grade Math				
Log Spending	-0.222	1.832	3.956	4.795	-0.222	1.846**	0.993**	0.691	0.023
	(1.737)	(2.307)	(4.747)	(3.074)	(1.737)	(0.656)	(0.418)	(0.370)	(0.540)
First-stage F	4.73	3.02	1.17	3.49	4.73	30.22	54.02	47.59	18.41
Districts	5,642	5,641	5,641	5,642	5,642	5,643	5,643	5,643	5,643
N	24,596	24,586	24,588	24,590	24,596	24,597	24,596	24,595	24,596
					D. 8th Grade Reading				
Log Spending	0.854	1.502	-1.588	0.424	0.854	0.502	0.490	0.506	0.236
	(1.353)	(1.726)	(3.297)	(1.168)	(1.353)	(0.350)	(0.260)	(0.270)	(0.381)
First-stage F	7.88	5.23	1.05	5.58	7.88	62.78	106.41	93.67	46.63
Districts	5,673	5,672	5,672	5,673	5,673	5,674	5,674	5,674	5,674
N	26,471	26,461	26,463	26,465	26,471	26,472	26,472	26,471	26,472

Notes: This table reports results from individual two-stage least squares regressions of test scores on lagged log total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.8: First stage estimates of simulated revenue ($1,000 per pupil) on total expenditure ($1,000 per pupil) for graduation rate samples – individual year lags

	$\tau = 0$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$	$\tau = 8$
	A. 8th Grade Cohort Graduation Rate								
Per-pupil Sim. Rev.	3.053**	2.768**	2.694**	2.099**	2.052**	0.905	0.289	-0.299	1.556
	(0.464)	(0.508)	(0.524)	(0.533)	(0.530)	(0.592)	(0.730)	(0.954)	(1.137)
F	43.20	29.67	26.47	15.49	14.98	2.34	0.16	0.10	1.87
Districts	2,724	2,724	2,722	2,721	2,720	2,716	2,708	2,688	2,668
N	17,467	17,467	17,275	16,967	15,677	14,640	13,203	11,510	9,147
	B. 9th Grade Cohort Graduation Rate								
Per-pupil Sim. Rev.	3.025**	2.687**	2.350**	2.139**	1.667**	0.476	0.658	0.896	1.537
	(0.468)	(0.442)	(0.428)	(0.435)	(0.468)	(0.559)	(0.695)	(0.898)	(1.016)
F	41.78	36.94	30.21	24.19	12.71	0.72	0.90	1.00	2.29
Districts	2,825	2,825	2,825	2,825	2,825	2,825	2,825	2,821	2,816
N	22,342	22,130	21,751	20,108	18,643	16,918	14,885	12,683	10,011
	C. 10th Grade Cohort Graduation Rate								
Per-pupil Sim. Rev.	3.198**	2.753**	2.318**	1.853**	0.956*	0.400	0.834	1.000	2.055*
	(0.426)	(0.419)	(0.443)	(0.456)	(0.475)	(0.541)	(0.656)	(0.901)	(1.016)
F	56.48	43.13	27.39	16.52	4.06	0.55	1.62	1.23	4.09
Districts	2,825	2,825	2,825	2,825	2,825	2,825	2,825	2,822	2,817
N	23,457	23,082	21,468	19,863	18,063	16,449	14,593	12,633	9,969
	D. 11th Grade Cohort Graduation Rate								
Per-pupil Sim. Rev.	3.224**	2.704**	2.346**	1.876**	1.000+	0.456	0.857	1.430	2.181*
	(0.413)	(0.423)	(0.431)	(0.491)	(0.542)	(0.650)	(0.755)	(0.915)	(1.082)
F	60.97	40.83	29.66	14.62	3.40	0.49	1.29	2.44	4.06
Districts	2,823	2,823	2,823	2,820	2,815	2,815	2,813	2,812	2,798
N	22,227	20,606	19,049	17,194	15,357	14,431	13,327	12,089	9,540

Notes: This table reports the results of individual first stage regressions of per-pupil total expenditures on log simulated revenue with the same lag for samples with non-missing graduation rates. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.9: First stage estimates of simulated revenue ($1,000 per pupil) on total expenditure ($1,000 per pupil) for test score samples – individual year lags

	$\tau = 0$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$	$\tau = 8$
A. SEDA 4th Grade Math Scores									
Per-pupil Sim. Rev.	1.053+	0.561	-0.157	1.010+	1.830**	1.877**	2.235**	1.977**	0.970*
	(0.552)	(0.549)	(0.609)	(0.597)	(0.494)	(0.444)	(0.404)	(0.480)	(0.480)
F	3.65	1.04	0.07	2.86	13.73	17.91	30.57	16.94	4.08
Districts	5,780	5,780	5,781	5,781	5,781	5,781	5,781	5,781	5,781
N	26,501	26,501	26,502	26,503	26,504	26,505	26,506	26,506	26,506
B. SEDA 4th Grade Reading Scores									
Per-pupil Sim. Rev.	1.190*	0.575	-0.125	1.053+	2.001**	1.970**	2.245**	2.020**	0.964*
	(0.531)	(0.548)	(0.610)	(0.596)	(0.489)	(0.441)	(0.406)	(0.473)	(0.479)
F	5.01	1.10	0.04	3.12	16.74	19.95	30.64	18.20	4.05
Districts	5,778	5,778	5,779	5,779	5,779	5,779	5,779	5,779	5,779
N	26,511	26,511	26,512	26,513	26,514	26,515	26,516	26,516	26,516
C. SEDA 8th Grade Math Scores									
Per-pupil Sim. Rev.	0.776	0.675	0.768	0.797	1.372**	0.912*	1.567**	1.832**	0.900+
	(0.526)	(0.574)	(0.640)	(0.640)	(0.493)	(0.431)	(0.435)	(0.550)	(0.526)
F	2.18	1.38	1.44	1.55	7.76	4.48	12.95	11.11	2.93
Districts	5,787	5,787	5,788	5,788	5,788	5,788	5,788	5,788	5,788
N	24,741	24,741	24,742	24,743	24,744	24,745	24,745	24,745	24,745
D. SEDA 8th Grade Reading Scores									
Per-pupil Sim. Rev.	1.129*	0.672	0.236	1.048+	1.906**	1.827**	2.182**	1.985**	0.996*
	(0.520)	(0.564)	(0.605)	(0.596)	(0.474)	(0.423)	(0.405)	(0.479)	(0.476)
F	4.70	1.42	0.15	3.10	16.18	18.68	29.09	17.15	4.38
Districts	5,801	5,801	5,802	5,802	5,802	5,802	5,802	5,802	5,802
N	26,599	26,599	26,600	26,601	26,602	26,603	26,604	26,604	26,604

Notes: This table reports the results of individual first stage regressions of per-pupil total expenditures on log simulated revenue with the same lag for samples with non-missing test scores. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.10: First stage estimates in $1,000 per pupil

	(1) 1 year	(2) 1-4 years	(3) 1-8 years	(4) 5-8 years
	A. 8th Grade Graduation Cohort Sample			
Per-pupil Sim. Rev.	0.010**	0.008+	0.003	-0.004
	(0.004)	(0.004)	(0.004)	(0.008)
F	5.87	3.43	0.90	0.26
Districts	2,885	2,881	2,820	2,821
N	18,610	16,570	9,552	9,553
	B. 9th Grade Graduation Cohort Sample			
Per-pupil Sim. Rev.	0.008*	0.009*	0.004	-0.002
	(0.004)	(0.004)	(0.004)	(0.008)
F	5.23	4.64	1.12	0.05
Districts	2,988	2,988	2,977	2,978
N	23,567	19,655	10,468	10,470
	C. 10th Grade Graduation Cohort Sample			
Per-pupil Sim. Rev.	0.009**	0.009*	0.002	-0.005
	(0.004)	(0.004)	(0.003)	(0.008)
F	5.80	4.54	0.26	0.41
Districts	2,988	2,987	2,979	2,980
N	24,538	19,082	10,440	10,442
	D. 11th Grade Graduation Cohort Sample			
Per-pupil Sim. Rev.	0.008**	0.007+	0.002	-0.007
	(0.004)	(0.004)	(0.004)	(0.008)
F	5.80	2.72	0.44	0.75
Districts	2,986	2,978	2,960	2,961
N	22,005	16,337	10,004	10,006
	E. SEDA Test Score Sample			
Per-pupil Sim. Rev.	0.001	-0.004	-0.003	-0.001
	(0.009)	(0.006)	(0.004)	(0.004)
F	0.00	0.48	0.61	0.02
Districts	5,951	5,951	5,951	5,952
N	25,534	25,534	25,534	25,538

Notes: This table reports the results of first stage regressions of total expenditures on simulated revenue averaged over various previous years. Column (1) is the current and previous year, column (2) is the current through past 4 years, column (3) is the past 8 years, and column (4) is from 5 to 8 years prior to the measured outcome. Models also include controls for property wealth, median household income, fraction black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: + $p < 0.1$ * $p < 0.05$, ** $p < 0.01$.

Table A.11: Two-stage least squares estimates of per-pupil spending on graduation rates

	$\tau = 0$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$	$\tau = 8$
				A. 8th Grade Cohort Graduation Rate					
Spending ($1,000s PP)	0.027**	0.027**	0.008	0.004	-0.011	0.000	-0.021	-0.048	-0.018
	(0.007)	(0.008)	(0.005)	(0.006)	(0.009)	(0.015)	(0.028)	(0.102)	(0.021)
Dep. Var. Mean	0.79	0.79	0.79	0.78	0.79	0.78	0.78	0.78	0.79
First-stage F	74.03	49.48	47.42	36.19	35.20	11.19	3.25	0.33	3.98
Districts	2,676	2,676	2,675	2,669	2,660	2,656	2,636	2,608	2,535
N	17,419	17,419	17,228	16,915	15,617	14,580	13,131	11,430	9,014
				B. 9th Grade Cohort Graduation Rate					
Spending ($1,000s PP)	0.019**	0.020**	0.006	0.005	-0.001	-0.007	-0.031	-0.036	-0.033
	(0.006)	(0.006)	(0.005)	(0.006)	(0.009)	(0.018)	(0.022)	(0.026)	(0.022)
Dep. Var. Mean	0.76	0.76	0.76	0.76	0.76	0.75	0.75	0.75	0.75
First-stage F	71.94	61.67	52.80	44.33	27.11	5.46	4.43	2.89	4.17
Districts	2,823	2,823	2,823	2,823	2,818	2,815	2,808	2,802	2,758
N	22,340	22,128	21,749	20,106	18,636	16,908	14,868	12,664	9,953
				C. 10th Grade Cohort Graduation Rate					
Spending ($1,000s PP)	0.013**	0.014**	0.005	0.009	0.011	0.018	-0.011	-0.029	-0.013
	(0.004)	(0.004)	(0.004)	(0.006)	(0.013)	(0.021)	(0.018)	(0.021)	(0.013)
Dep. Var. Mean	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.80	0.81
First-stage F	99.92	74.71	47.84	32.36	13.71	4.97	5.70	3.26	6.63
Districts	2,824	2,824	2,824	2,824	2,822	2,822	2,818	2,804	2,773
N	23,456	23,081	21,467	19,862	18,060	16,446	14,586	12,615	9,925
				D. 11th Grade Cohort Graduation Rate					
Spending ($1,000s PP)	0.009**	0.013**	0.005	0.010	0.009	0.012	-0.009	-0.029*	-0.022
	(0.003)	(0.004)	(0.004)	(0.007)	(0.014)	(0.022)	(0.019)	(0.017)	(0.014)
Dep. Var. Mean	0.85	0.85	0.85	0.85	0.85	0.84	0.85	0.86	0.86
First-stage F	110.92	70.80	50.31	28.89	11.32	3.51	4.48	5.03	6.43
Districts	2,820	2,819	2,815	2,810	2,803	2,794	2,786	2,773	2,722
N	22,224	20,602	19,041	17,184	15,345	14,410	13,300	12,050	9,464

Notes: This table reports results from individual two-stage least squares regressions of graduation rates on individually lagged per-pupil total expenditures instrumented with individually lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.12: Two-stage least squares estimates of per-pupil spending on graduation rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	8th Grade Cohort		9th Grade Cohort		10th Grade Cohort		11th Grade Cohort	
	1 year	1-4 years	1 year	1-4 years	1 year	1-4 years	1 year	1-4 years
Spending ($1,000s per pupil)	0.028**	0.018**	0.020**	0.021**	0.015**	0.026**	0.013**	0.027**
	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)	(0.007)	(0.004)	(0.008)
Dep. Var. Mean	0.79	0.79	0.76	0.76	0.81	0.81	0.85	0.85
First-stage F	70.42	85.94	72.96	79.86	83.60	67.40	80.64	55.29
Districts	2,676	2,660	2,823	2,817	2,824	2,821	2,819	2,802
N	17,419	15,616	22,128	18,633	23,081	18,058	20,602	15,342

Notes: This table reports results from individual two-stage least squares regressions of graduation rates on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.13: Two-stage least squares estimates of per-pupil spending on test scores

	$\tau = 0$	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$	$\tau = 8$
A. SEDA 4th Grade Math Scores									
Spending ($1,000s PP)	-0.176	-0.081	-0.675	0.064	0.073	0.096*	0.065+	0.093+	0.164
	(0.133)	(0.148)	(2.695)	(0.103)	(0.050)	(0.047)	(0.035)	(0.050)	(0.122)
Dep. Var. Mean	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
First-stage F	5.09	2.13	0.07	2.98	13.89	18.29	31.05	17.20	4.29
Districts	5,663	5,663	5,663	5,664	5,664	5,664	5,664	5,664	5,664
N	26,384	26,384	26,384	26,386	26,387	26,388	26,389	26,389	26,389
B. SEDA 4th Grade Reading Scores									
Spending ($1,000s PP)	-0.071	-0.182	-1.179	0.083	0.115*	0.087*	0.092**	0.118**	0.132
	(0.092)	(0.170)	(5.704)	(0.101)	(0.050)	(0.042)	(0.034)	(0.047)	(0.105)
Dep. Var. Mean	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
First-stage F	6.75	2.21	0.04	3.24	16.92	20.35	31.12	18.48	4.26
Districts	5,660	5,660	5,660	5,661	5,661	5,661	5,661	5,661	5,661
N	26,393	26,393	26,393	26,395	26,396	26,397	26,398	26,398	26,398
C. SEDA 8th Grade Math Scores									
Spending ($1,000s PP)	-0.016	0.100	0.190	0.297	0.158*	0.251+	0.102*	0.079+	0.003
	(0.125)	(0.125)	(0.219)	(0.255)	(0.076)	(0.135)	(0.049)	(0.048)	(0.079)
Dep. Var. Mean	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11
First-stage F	3.49	2.63	1.44	1.65	7.90	4.73	13.30	11.33	3.12
Districts	5,642	5,642	5,642	5,643	5,643	5,643	5,643	5,643	5,643
N	24,596	24,596	24,596	24,598	24,599	24,600	24,600	24,600	24,600
D. SEDA 8th Grade Reading Scores									
Spending ($1,000s PP)	0.060	0.122	-0.245	0.026	0.060	0.053	0.052+	0.069+	0.047
	(0.095)	(0.141)	(0.761)	(0.079)	(0.037)	(0.038)	(0.029)	(0.041)	(0.081)
Dep. Var. Mean	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24
First-stage F	6.40	2.60	0.15	3.22	16.35	19.08	29.56	17.41	4.60
Districts	5,673	5,673	5,673	5,674	5,674	5,674	5,674	5,674	5,674
N	26,471	26,471	26,471	26,473	26,474	26,475	26,476	26,476	26,476

Notes: This table reports results from individual two-stage least squares regressions of SEDA test scores on individually lagged per-pupil total expenditures instrumented with individually lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.14: Two-stage least squares estimates of per-pupil spending on test scores

	(1)	(2)	(3)	(4)
	4th Grade		8th Grade	
	Math	Reading	Math	Reading
	A. SEDA Test Scores			
Spending ($1,000s PP)	0.091*	0.101**	0.110*	0.057+
	(0.042)	(0.038)	(0.052)	(0.034
First-stage F	46.12	49.28	21.80	45.64
Districts	5,664	5,661	5,643	5,674
N	26,388	26,397	24,600	26,475

Notes: This table reports results from individual two-stage least squares regressions of SEDA test scores on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.15: Two-stage least squares estimates of log spending on instructional expenditure sub-categories – Graduation Rate sample, one-year lag

A. 10th Grade Cohort Graduation Rate Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Instructional	Teacher Salaries				Instructional
	Salaries	Regular	Special	Vocational	Other	Benefits
Spending ($1,000s PP)	0.277**	0.559**	0.095**	0.023**	0.041**	0.126**
	(0.030)	(0.063)	(0.013)	(0.004)	(0.006)	(0.014)
Dep. Var. Mean	3.92	1.49	0.29	0.05	0.07	1.174
Baseline Fraction	0.32	0.12	0.02	0.00	0.01	0.094
First-stage F	83.60	83.60	83.60	83.60	83.60	83.60
Districts	2,824	2,824	2,824	2,824	2,824	2,824
N	23,081	23,081	23,081	23,081	23,081	23,081

B. SEDA Test Score Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Instructional	Teacher Salaries				Instructional
	Salaries	Regular	Special	Vocational	Other	Benefits
Spending ($1,000s PP)	0.415**	0.941**	0.206**	-0.028**	0.044**	0.166**
	(0.086)	(0.202)	(0.046)	(0.007)	(0.011)	(0.034)
Dep. Var. Mean	3.88	1.69	0.32	0.05	0.07	1.155
Baseline Fraction	0.34	0.16	0.03	0.01	0.01	0.117
First-stage F	21.42	21.42	21.42	21.42	21.42	21.42
Districts	5,527	5,527	5,527	5,527	5,527	5,527
N	24,006	24,006	24,006	24,006	24,006	24,006

Notes: This table reports results from individual two-stage least squares regressions of per-pupil instructional expenditure sub-categories on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.16: Two-stage least squares estimates of per-pupil spending on support service expenditure sub-categories

A. 10th Grade Cohort Graduation Rate Sample

	(1) Pupil Support	(2) Staff Support	(3) General Admin.	(4) School Admin.	(5) Operations & Maintenance	(6) Transportation	(7) Other
Spending ($1,000s PP)	0.058**	0.068**	0.009*	0.039**	0.087**	0.031**	0.030**
	(0.007)	(0.009)	(0.004)	(0.005)	(0.011)	(0.005)	(0.005)
Dep. Var. Mean	0.49	0.38	0.24	0.49	0.87	0.426	0.246
Baseline Fraction	0.04	0.03	0.02	0.04	0.07	0.034	0.020
First-stage F	83.60	83.60	83.60	83.60	83.60	83.60	83.60
Districts	2,824	2,824	2,824	2,824	2,824	2,824	2,824
N	23,081	23,081	23,081	23,081	23,081	23,081	23,081

B. SEDA Test Score Sample

	(1) Pupil Support	(2) Staff Support	(3) General Admin.	(4) School Admin.	(5) Operations & Maintenance	(6) Transportation	(7) Other
Spending ($1,000s PP)	0.074**	0.039**	0.031**	0.047**	0.070**	0.020**	0.029**
	(0.017)	(0.012)	(0.013)	(0.011)	(0.018)	(0.008)	(0.008)
Dep. Var. Mean	0.46	0.36	0.30	0.48	0.87	0.444	0.234
Baseline Fraction	0.04	0.03	0.03	0.04	0.08	0.041	0.024
First-stage F	21.42	21.42	21.42	21.42	21.42	21.42	21.42
Districts	5,527	5,527	5,527	5,527	5,527	5,527	5,527
N	24,006	24,006	24,006	24,006	24,006	24,006	24,006

Notes: This table reports results from individual two-stage least squares regressions of per-pupil support service expenditure sub-categories on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.17: Two-stage least squares estimates of per-pupil spending on capital expenditure sub-categories

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Graduate Rates					SEDA Test Scores				
	New Construction	Land	Instructional Equipment	Other Equipment	Nonspecified Equipment	New Construction	Land	Instructional Equipment	Other Equipment	Nonspecified Equipment
Spending ($1,000s PP)	0.234**	0.012	0.006*	0.004	0.0000	0.251**	-0.024	0.013**	-0.006	0.0016
	(0.049)	(0.015)	(0.003)	(0.007)	(0.001)	(0.087)	(0.031)	(0.004)	(0.011)	(0.005)
Dep. Var. Mean	0.79	0.063	0.057	0.13	0.011	0.79	0.056	0.058	0.13	0.011
Baseline Fraction	0.05	0.004	0.005	0.01	0.001	0.05	0.004	0.005	0.01	0.001
First-stage F	83.60	83.60	83.60	83.60	83.60	21.42	21.42	21.42	21.42	21.42
Districts	2,824	2,824	2,824	2,824	2,824	5,527	5,527	5,527	5,527	5,527
N	23,081	23,081	23,081	23,081	23,081	24,006	24,006	24,006	24,006	24,006

Notes: This table reports results from individual two-stage least squares regressions of per-pupil capital expenditure sub-categories on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.18: Two-stage least squares estimates of per-pupil spending on other current expenditure sub-categories

A. 10th Grade Cohort Graduation Rate Sample

	(1) Food Services	(2) Enterprise Operations	(3) Other Elem/Sec	(4) Community Services	(5) Adult Education	(6) Other Non-Elem/Sec
Spending ($1,000s PP)	0.012**	0.000	0.001	0.003	-0.002	0.042**
	(0.003)	(0.001)	(0.001)	(0.003)	(0.002)	(0.008)
Dep. Var. Mean	0.35	0.021	0.0026	0.031	0.017	0.010
Baseline Fraction	0.03	0.002	0.0002	0.003	0.001	0.001
First-stage F	83.60	83.60	83.60	83.60	83.60	83.60
Districts	2,824	2,824	2,824	2,824	2,824	2,824
N	23,081	23,081	23,081	23,081	23,081	23,081

B. SEDA Test Score Sample

	(1) Food Services	(2) Enterprise Operations	(3) Other Elem/Sec	(4) Community Services	(5) Adult Education	(6) Other Non-Elem/Sec
Spending ($1,000s PP)	-0.019**	0.002*	0.002	0.003	-0.005+	0.001
	(0.005)	(0.001)	(0.002)	(0.004)	(0.002)	(0.001)
Dep. Var. Mean	0.34	0.025	0.0028	0.029	0.015	0.005
Baseline Fraction	0.03	0.002	0.0001	0.002	0.001	0.001
First-stage F	21.42	21.42	21.42	21.42	21.42	21.42
Districts	5,527	5,527	5,527	5,527	5,527	5,527
N	24,006	24,006	24,006	24,006	24,006	24,006

Notes: This table reports results from individual two-stage least squares regressions of per-pupil other current expenditure sub-categories on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.19: Two-stage least squares estimates of spending on teacher counts

A. 10th Grade Cohort Graduation Rate Sample

	(1) Total Teachers	(2) Teacher Aides	(3) Total Counselors	(4) Library Specialists	(5) District Admin.	(6) School Admin.	(7) Student Support
Spending ($1,000s PP)	-15.715**	-2.259	-1.334**	-0.054	-0.152	-2.033**	-1.223
	(6.687)	(1.718)	(0.509)	(0.103)	(0.223)	(0.776)	(1.284)
Dep. Var. Mean	337.38	70.186	11.6710	6.304	6.519	18.041	21.469
Baseline Fraction	28.29	6.01	0.99	0.54	0.56	1.56	1.84
First-stage F	83.60	83.60	83.60	83.60	83.60	83.60	83.60
Districts	2,824	2,824	2,824	2,824	2,824	2,824	2,824
N	23,081	23,081	23,081	23,081	23,081	23,081	23,081

B. SEDA Test Score Sample

Notes: This table reports results from individual two-stage least squares regressions of teacher counts on lagged per-pupil total expenditures instrumented with lagged log simulated revenue. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

Table A.20: Two-stage least squares estimates of log spending, other specifications

A. National Trends

	(1)	(2)	(3)	(4)	(5)
	10th Grade Graduation Rate	Test Scores: 5 to 8 Year Lag			
	1-Year Lag	G4 Math	G4 Reading	G8 Math	G4 Reading
Log Spending	0.250**	-4.973	-4.117	-11.836	-1.552
	(0.077)	(9.799)	(8.195)	(34.710)	(5.915)
F	65.85	0.45	0.48	0.14	0.50
Districts	5,447	5,662	5,659	5,640	5,671
N	35,442	26,372	26,381	24,582	26,458

B. No Combined Districts

	(1)	(2)	(3)	(4)	(5)
	10th Grade Graduation Rate	Test Scores: 5 to 8 Year Lag			
	1-Year Lag	G4 Math	G4 Reading	G8 Math	G4 Reading
Log Spending	0.270**	0.636+	0.706*	1.002**	0.593*
	(0.080)	(0.345)	(0.309)	(0.411)	(0.281)
F	76.60	176.59	178.50	95.19	173.59
Districts	2,628	5,224	5,221	5,197	5,229
N	21,444	24,428	24,437	22,647	24,475

C. No Combined Districts + No Sample Restrictions

	(1)	(2)	(3)	(4)	(5)
	10th Grade Graduation Rate	Test Scores: 5 to 8 Year Lag			
	1-Year Lag	G4 Math	G4 Reading	G8 Math	G4 Reading
Log Spending	0.290**	0.475	0.253	0.005	0.085
	(0.085)	(0.301)	(0.240)	(0.479)	(0.220)
F	72.38	132.47	138.79	45.83	142.72
Districts	2,655	5,470	5,474	5,431	5,471
N	21,829	29,881	30,086	26,770	29,556

Notes: This table reports results from two-stage least squares regressions of graduation rates and test scores on average lagged log total expenditures instrumented with log simulated revenue, averaged over the same years. Models also include controls for district property wealth, median household income, fraction of students who are black, fraction Hispanic, fraction special education, fraction eligible for free or reduced price lunch, district fixed effects, and state-by-year fixed effects. All covariates are interacted with an indicator equal to 1 if the fraction of students in the district eligible for free or reduced-price lunch is in the top quartile for their state. Standard errors are clustered at the district level: * $p < 0.05$, ** $p < 0.01$.

A.2 Online Appendix - State Administrative Data

This section outlines the data sources for property values from each state.

A.2.1 Arkansas

Tables with assessed valuation, tax rate, and taxes levied by class of property for counties, cities, and school districts are available from the Arkansas Assessment Coordination Department.[1] School district-level values are available from 1995-2010, but they have not compiled school district-level values since 2010. The assessment rate for real property in Arkansas is 0.2, so my measure of market values is the assessed value multiplied by 5.

A.2.2 Connecticut

The Connecticut Office of Policy and Management (OPM) publishes excel sheets of the grand list by town on their website. The description from the website reads: "The Office of Policy and Management annually develops the full-value estimate of all taxable property within the 169 towns and cities. A ratio of assessment to market value is calculated from real estate sales occurring within each town and city. A separate ratio and full-value estimate is listed for four property types: residential, apartment, commercial/industrial/public utility and vacant land." Thus, the net grand list is the value determined by the assessors and the net equalized grant list corrects any inconsistencies in the assessment process to arrive at their best estimate

[1] http://www.arkansas.gov/acd/statewide_values_rates_assessed_values.html

of the market value in that town or city. The net equalized grand list subtracts out exemptions, so the full market value is the gross equalized grand list. This is readily available in the Total Grand List files, which are available for 1995-2012 because they also report the equalized value of exemptions. I only require the accurate levels of property values in the base year, so I use the 2013 and 2014 data to calculate the state-level changes in property wealth from the net equalized grand list to supplement the information from the gross equalized grand list in prior years.

The towns and cities perfectly map into school district boundaries, with each school district covering a set of towns and cities. Thus, I aggregate the city-level file from the OPM to the school district level. Real property must be reevaluated once every decade, and at that time, it is assessed at 70% of fair market value (FMV). Personal property is also assessed at 70% of FMV, but this FMV is adjusted annually. Property values are limited to changes of 5% between any two years.

A.2.3 Florida

The Florida Department of Revenue provides property tax data going back several decades (historical data is available on their website: ftp://sdrftp03.dor.state.fl.us/DataBooks). The data is part of the "Florida Ad Valorem Valuations and Tax Data" series. Table 4 reports "just values" (their term for full market values) for real, personal, and centrally assessed property combined. I use these just values as the total property wealth in the county. School districts are coterminous with counties so the county-level information is matched directly to school districts.

A.2.4 Georgia

The Local Government Services Division of the Georgia Department of Revenue is charged with ensuring that property taxes are assessed uniformly and administered properly by each of the county tax officials. They provide a digest of property tax values on their website.[2] These digests include values for each class of property for each county and independent school district.

A.2.5 Idaho

The Idaho State Department of Education has a report in their archives[3] about school district property taxes back to 1999. These reports include the market value of property by school district, tax rates by purpose by school district, taxes levied by purpose by school district. I match this market value of property directly to school districts.

A.2.6 Illinois

The Illinois Department of Revenue publishes a report called "Illinois Property Tax Statistics", which is available on their website[4] back to 1976. This report includes the total equalized assessed value for each taxing district in Table 28. I apply the inverse of the assessment rate to the assessed value to retrieve the total market value in each district. This file includes information for every taxing district including

[2] https://dor.georgia.gov/digest-consolidated-summaries
[3] http://www.sde.idaho.gov/finance/archives.html
[4] http://tax.illinois.gov/AboutIdor/TaxStats/PropertyTaxStats/PreviousYears/

school districts, which are then matched to the school district information from the CCD.

A.2.7 Iowa

The Iowa Department of Management has information about school property taxes back to 2003 on their website.[5] The data includes net valuations by school district (2001-2016), tax rates by purpose by school district (2001-2016), total levies by school district (2003-2016), instructional support levies by school district (2003-2016), physical plant and equipment levies by school district (2003-2016), tax rates and revenue by county (2002-2016).

A.2.8 Kansas

The Kansas Department of Revenue produces an annual statistical report, which it makes available on its website back to 2004. These reports include a summary report of assessed property values by school district, compiled by the Division of Property Valuation. These reports are available from 2001 to 2015, but the information reported is from the previous year so corresponds to values from 2000-2014. Kansas has three separate assessment rates for five classes of property. Residential property is assessed at 11.5 percent of market value. Commercial real estate, motor vehicles, and agricultural property are each assessed at 30 percent of market value. Commercial equipment and machinery is assessed at 20 percent of market value. The

[5] http://www.dom.state.ia.us/local/schools/archive.html

majority of property assessed is real property so I use the residential assessment rate to convert the assessed values into approximate market values.

A.2.9 Kentucky

The Kentucky Revenue Cabinet releases state-level statistics on property tax revenues annually. The Kentucky Department of Revenue Office of Property Valuation has an archive of tax rates of each county by purpose (including education) and class, and tax rates for each school district by class on their website[6] back to 1999. On the individual county information pages there is valuation information by county for the whole state from 2005-2015, which was supplemented with years 1999-2004 by contacting the department directly.

A.2.10 Massachusetts

The Massachusetts Department of Revenue publishes an annual report available on their website[7] back to 2003. This report includes statewide summaries of revenues collected. The Division of Local Services has oversight of property taxation and municipal finance. Their website[8] also has a Municipal Databank with taxable values by municipality (back to 1981), assessed values by class by municipality (back to 1981), taxes levied by class by municipality (back to 1986), school versus total expenditure

[6] http://revenue.ky.gov/NR/exeres/ADD5DAC1-5E46-4DCD-B3C0-E54DBA6D8E05,frameless.htm?NRMODE=Published

[7] http://www.mass.gov/dor/tax-professionals/news-and-reports/annual-reports/

[8] http://www.mass.gov/dor/local-officials/municipal-databank-and-local-aid-unit/databank-reports-new.html

(back to 1986), and tax rate by class by municipality (back to 1981).

A.2.11 Minnesota

Each year, the Department of Revenue reports to the Legislature on property tax values and assessment practices in Minnesota. Posted on their website[9], these reports analyze market trends, the effects of property tax laws and changes to them, and how property values are assessed throughout the state. These reports were mandated by the Legislature in 2001, and the first one was issued in 2003. These reports have county by county market values by class and the last decade of changes to market value compared to the statewide average.

A.2.12 Mississippi

The Mississippi State Tax Commission was restructured in 2010 to become the Department of Revenue. The DOR presents an annual report of financial and statistical data pertaining to tax collections in the state of Mississippi for the fiscal year (July–June). The Department of Archives and History has electronic (PDF) reports from 2003. These reports include the assessment of property by class by county (back to 2002). Millage rate reports by county are available here back to 2010.

[9] http://www.revenue.state.mn.us/propertytax/Pages/apreport.aspx

A.2.13 Nevada

The Nevada Department of Taxation publishes an annual report available on their website[10] back to 2005 (state archive[11] has reports back to 1998). These reports include assessed value after exemptions by county (taxable value). There is an additional report on Property Tax Rates available on the website[12] back to 1971. These reports include assessed values and total property tax rates by taxing unit, combined property tax rates by component by taxing unit (taxing units include counties, municipalities, and school districts).

A.2.14 New Hampshire

The New Hampshire Department of Revenue Administration is statutorily required to issue an Annual Report, which is available on their website[13] back to 2002.

A.2.15 New Jersey

The State of New Jersey Department of Community Affairs has data available on its website[14] for 1998-2015. The property tax tables include taxable value, taxes levied by purpose, and tax rates by purpose all by municipality. The Property Value Classification tables include additional valuation details including residential values and the percentage of total value from residential property.

[10] http://tax.nv.gov/Publications/Annual_Report/

[11] http://www.nsladigitalcollections.org/browse/taxationstateofnevada#search/facet_1:Annual%20Reports/

[12] http://tax.nv.gov/LocalGovt/PolicyPub/ArchiveFiles/Redbook/

[13] http://revenue.nh.gov/publications/reports/index.htm

[14] http://www.state.nj.us/dca/divisions/dlgs/resources/property_tax.html

A.2.16 New Mexico

The New Mexico Taxation and Revenue Department releases "Property Tax Facts", which are available on their website[15] back to 2004 (Economic and Statistical Information/Property Tax Reports). These fact sheets include residential taxable values and obligations by county (Table 1), property taxes by purpose by county (Table 4).

A.2.17 New York

The overall full-value tax rates are available by county on their website . The New York State Department of Taxation and Finance has several annually published reports regarding property taxes on their website.[16] One of these reports is titled "Exemptions from Real Property Taxation in New York State" and includes detailed exemption data by county and municipality (back to 2000) including total equalized value (Table B1).

Useful data is also available at data.ny.gov, including real property tax levy data (back to 2004).

A.2.18 North Carolina

The North Carolina Department of Revenue makes several tables of statistics available on their website . These reports include effective tax rates for counties and municipalities (back to 2002-2003 here), real property valuations by class by county

[15] http://www.tax.newmexico.gov/forms-publications.aspx

[16] https://www.tax.ny.gov/pubs_and_bulls/orpts/publications/property_pubs_prior.htm

(back to 2003-2004 here), property valuations by real, personal, and public service by county (back to 1998-1999 here). The North Carolina Department of State Treasurer also has reports on their website . These include assessed valuation, tax rate, assessment ratio, effective tax rate by county and municipality back to 2000.

A.2.19 North Dakota

The North Dakota Office of State Tax Commissioner produces several major publications available on their website . These include the State and Local Taxes Guide (biennially back to 1998) with general property taxes levied by county. Another report titled Property Valuations and Property Taxes Levied in North Dakota/The Property tax Statistical Report includes taxable valuation by class by county (Table 1), general and special property taxes levied by county by political subdivision (Table 3 – includes school districts), property taxes levied by tax code by county (Table 4), taxes levied on classes of property by county (Table 5), and millage rates by tax code by county (Table 7).

North Dakota has a 9 percent assessment rate on property. I use this to convert the assessed values reported in the financial facts document to approximate market values.

A.2.20 Ohio

The Ohio Department of Taxation makes data available on their website . These data include taxable property values, taxes levied, and tax rates by school district

(SD1, back to 1986); millage rates by school district (DTE27, back to 1994); assessed value and taxes levied by county (PD 30, back to 1987); and taxable value of real property by class by county (PD31, back to 1985).

A.2.21 Oklahoma

Oklahoma uses a range of assessment rates for multiple classes of property. The assessment rate for real property varies between 11 percent and 13.5 percent. Personal property is assessed between 10 and 15 percent of market value. Public service property is assessed at a fixed rate of 22.85 percent. I do not have information on which assessment rate is used in each district. Thus, I use the lower bound of the ranges of assessment rates when converting assessed values into approximate market values.

A.2.22 Oregon

The Oregon Department of Revenue publishes an annual report titled Oregon Property Tax Statistics, which is available on their website back to FY 1997-1998. These reports include market and assessed value, taxes levied, and average tax rate by county (all property, Table A.2); market value and assessed value by type (real, personal, etc.) by county (Table A.4); assessed value by class by county (Table B.4); tax rates by type of taxing district by city (Table H); and tax rates by type of tax by taxing district (including school districts, Appendix A).

A.2.23 Texas

The Texas Comptroller of Public Accounts Property Tax Division publishes an Annual Property Tax Report, which is available on their website back to 2003. These reports have several tables of information including appraised values by category by appraisal district (Appendix A); appraised values by category, millage rates, and taxes levied by school district (Appendix B, I somehow have Appendix B for 2002 as well); appraised values by category, millage rates by purpose and associated taxes levied by county (Appendix C). The key data comes from the School District Self-Reports.

A.2.24 Washington

The Washington State Department of Revenue releases an annual Tax Statistics report, which is archived on their website back to 1997. These are available as full pdf reports back to 2006 and excel files of the tables are available from 2001-2015. There is also a Property Tax Statistical Report available here back to 1998, with a table of taxable value and levies due by school district (Part 3/Appendix C).

A.3 Online Appendix - School Finance Formulas

This section outlines the school finance formulas for each state in my sample and summarizes them as the tax price and wealth price as of 1999. It is helpful to establish some notation used throughout this section. Here I will call ℓ_t^d the district markup, which is a multiplicative factor that encompasses statewide assessment ratios and district-specific factors that determines the amount of revenue given property wealth and the tax rate (e.g. fraction of homes receiving a homestead exemption, rates of delinquent property tax payments, etc.) such that $\tau_t^d \times \ell_t^d W_t^d = L_t^d(\tau_t^d \times W_t^d)$.

A.3.1 Arkansas

Arkansas has a foundation program, which is distributed as State Equalization Funding per Student (SEFPS) and Additional Base Funding (ABF) (Ark. Stat. Ann. §6-20-303). Every school district must tax itself 25 mills for Maintenance and Operation and half the revenue from each additional mill above 25 is captured by the state.

State Equalization Funding per Student (SEFPS) for each district is calculated by subtracting each district's local revenue per student (LRPS) from the basic local revenue per student (BLRPS), which gives

$$SEFPS_t^d = \frac{0.98\times(0.025\times\ell_t^s W_t^s)+0.75\times\text{Misc.}_t^s}{ADM_t^s} - \frac{0.98\times(0.025\times\ell_t^d W_t^d)+0.75\times\text{Misc.}_t^d}{ADM_t^d},$$

where W_t^s is the aggregate property assessment for the state, Misc._t^s is the aggregate miscellaneous and other funds from state sources, and ADM_t^s is the aggregate average daily membership for the state, and corresponding variables with a d superscript are the same measures but at the district level.

Additional Base Funding (ABF) combines revenue sources on an ADM basis and brings all school districts up to a minimum level of revenue per ADM. The revenues included in the calculation of ABF are: Total Local M&O Revenue Available, State Equalization Funding, General Facilities Funding, Student Growth Funding, and Revenue Loss Funding. These revenue sources are totaled and divided by the ADM of the district. Once the total state and local revenue per ADM is calculated, all of the school districts are ranked from most revenue per ADM to the least revenue per ADM. The revenue per ADM for the school district at the 95th percentile is multiplied by 80% to arrive at the Minimum State and Local Revenue per ADM (MSLR). Any school district whose revenue per ADM is less than the MSLR receives ABF in the amount per ADM equal to the difference between the district's revenue per ADM and MSLR. Revenue per ADM_t^d is

$$\frac{R_t^d}{ADM_t^d} = \frac{\left(\frac{\tau_t^d - 0.025}{2} + 0.025\right) \times \ell_t^d W_t^d + \text{Misc.}_t^d}{ADM_t^d} + SEFPS_t^d$$

where here Misc._t^d includes General Facilities Funding, Student Growth Funding, and Revenue Loss Funding. The Minimum State and Local Revenue per ADM is

$$MSLR_t^d = 0.80 \times P_{95}\left(\frac{R_t^d}{ADM_t^d}\right)$$

where $P_{95}\left(\frac{R_t^d}{ADM_t^d}\right)$ is the 95th percentile of $\frac{R_t^d}{ADM_t^d}$ for all $d \in D$. Using these definitions, the Additional Base Funding is

$$ABF_t^d = \max\left(0, MSLR_t^d - \frac{R_t^d}{ADM_t^d}\right)$$

For my revenue functions, I omit miscellaneous revenues since they are unrelated to property wealth and so will not effect the wealth price. Thus, local revenue is

$$L_t^d = \begin{cases} \tau_t^d \times \ell_t^d W_t^d, & \text{if } \tau_t^d < 0.025 \\ (0.5(\tau_t^d - 0.025) + 0.025) \times \ell_t^d W_t^d, & \text{if } \tau_t^d \geq 0.025 \end{cases}$$

state revenue is

$$S_t^d = (ABF_t^d + SEFPS_t^d) \times ADM_t^d$$

and total revenue is

$$R_t^d = (0.5(\tau_t^d - 0.025) + 0.025) \times \ell_t^d W_t^d + (ABF_t^d + SEFPS_t^d) \times ADM_t^d$$

The wealth price is then given by

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.025) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} 0 & \text{if } \frac{R_t^d}{ADM_t^d} < 0.8P_{95}\left(\frac{R_t^d}{ADM_t^d}\right) \\ 0.5\ell_t^d W_t^d & \text{if } \frac{R_t^d}{ADM_t^d} \geq 0.8P_{95}\left(\frac{R_t^d}{ADM_t^d}\right). \end{cases}$$

A.3.2 Connecticut

School districts in Connecticut are financed by their townships instead of being independent taxing authorities. Thus, their finance system is quite simple. The state determines how much money per weighted student the district should get, then decides how much they will pay and how much needs to be covered by the town. Townships set a single millage rate for all their local revenue, including for school districts, so it's impossible to tell the effect of marginally increasing property tax rates. Connecticut's Education Cost Sharing (ECS) is a foundation-based equalization formula that distributes aid based on the extent to which local town wealth falls short of a statutorily set State Guaranteed Wealth Level (SGWL).

The unit of allocation of the ECS is "need students" Student counts are weighted as follows to arrive at total need students. The resident student count of each town (ADM) is the number of children educated at the expense of the town in public schools or in other placements prescribed and paid for by the town. A full-time equivalent count (FTE) is added to the resident student count if a district operates either a system-wide calendar in excess of 180 days (the legal minimum) or a free summer school program or both. Additional weights include 25% for students in families eligible for TANF, 10% for students with limited English proficiency (LEP), and 25% for students at remedial education levels (Remedial). Need students can then be defined as

$$NS_t^d = ADM_t^d + FTE_t^d + 0.25 \times TANF_t^d + 0.1 \times LEP_t^d + 0.25 \times Remedial_t^d.$$

Local fiscal capacity is determined by town wealth per student. The ECS definition of town wealth begins with each town's Equalized Net Grand List (ENGL). ENGL per pupil is then adjusted based on the average ratio of each town's per capita income (PCI) and median household income (MHI) to the highest town's PCI and MHI. Income-adjusted property wealth is given by:

$$AdjW_t^d = \frac{1}{2} \times \left(\frac{PCI_t^d}{\max_{i \in D}\{PCI_t^i\}} + \frac{MHI_t^d}{\max_{i \in D}\{MHI_t^i\}} \right) \times W_t^d$$

$AdjW_t^d$ is then divided by need students and by populations and the average of these two is the town's unit value of equalized taxable property wealth. Equalized wealth is given by the average of adjusted wealth divided by need students and adjusted wealth divided by population. That is,

$$EW_t^d = \frac{1}{2} \times \left(\frac{AdjW_t^d}{NS_t^d} + \frac{AdjW_t^d}{Pop_t^d} \right)$$

Each town's equalized wealth is compared to 1.55 times the median town's wealth, which is called the state guaranteed wealth level (SGWL). Specifically,

$$SGWL_t = 1.55 \times \text{median}_{i \in D}\{EW_t^i\}$$

A town's ability to pay is reflected by its wealth as a fraction of the SGWL. Towns with local resources equal to or above the SGWL receive a base aid percentage of zero. All others receive the difference between 100% and the percentage they are able to fund based on the fraction described above. This percentage is then multiplied times the town's total foundation which is the product of the foundation and the total need students of the town. The local share of this town's total basic foundation is

$$\frac{EW_t^d}{SGWL_t^d} \times NS_t^d \times Foundation_t$$

The foundation for 1998-99 is set by statute at $5,775 per need student. This makes the local revenue function

$$L_t^d = \ell_t^d \tau_t^d W_t^d$$

the state revenue function is

$$S_t^d = \left(1 - \frac{EW_t^d}{SGWL_t^d}\right) \times NS_t^d \times 5775$$

and total revenue is

$$R_t^d = ETR_t^d \times W_t^d + \left(1 - \frac{EW_t^d}{SGWL_t^d}\right) \times NS_t^d \times 5775.$$

The wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = ETR_t^d - \frac{1}{4} \times \left(\frac{PCI_t^d}{\max_{i\in D}\{PCI_t^i\}} + \frac{MHI_t^d}{\max_{i\in D}\{MHI_t^i\}}\right) \times \left(1 + \frac{NS_t^d}{Pop_t^d}\right) \times \frac{1}{SGWL_t^d} \times 5775$$

and the tax price is unclear because there is no separate tax rate for education.

A.3.3 Florida

The Florida Education Finance Program (FEFP) is a highly-modified foundation plan laid out in Florida Statute §236.081. Funds are distributed based on weighted full time equivalents (WFTE) multiplied by the foundation amount (called the base student allocation or BSA) and the District Cost Differential (DCD). A number of other funds are included such as the Declining Enrollment Supplement, the Sparsity Supplement, Discretionary Tax Equalization, the Safe Schools Allocation, the Remediation Reduction Incentive, the Dropout Prevention Incentive, the Disparity Compression Adjustment, and the Hold Harmless Adjustment. Only the Sparsity Supplement and Discretionary Tax Equalization interact with W_t^d so I omit the other items.

The foundation tax rate is called Required Local Effort Millage (RLE Millage$_t^d$) and is set at 6.509 mills, then adjusted by an equalization factor for each district. This equalization factor is based on the relative amount of property wealth in the district compared to the rest of the state.

WFTE is calculated using the program cost factors outlined in Table A.21. Using these cost factors WFTE is

$$\begin{aligned} WFTE_t^d &= 1.057 \times FTE_t^{k-3} + FTE_t^{4-8} + 1.138 \times FTE^{9-12} + 1.201 \times ESOL_t \\ &+ 1.240 \times VOC_t + 1.341 \times SL1_t + 2.072 \times SL2_t + 3.287 \times SL3_t \\ &+ 4.101 \times SL4_t + 6.860 \times SL5_t \end{aligned}$$

Table A.21: Florida Pupil Weights

Group	Weight
Basic Programs	
Kindergarten and Grades 1, 2, and 3	1.057
Grades 4, 5, 6, 7, and 8	1.000
Grades 9, 10, 11, and 12	1.138
Programs for At-Risk Students	
Dropout Prevention and Teenage Parent	1.399
Educational Alternatives, Grades 9–12	1.138
Intensive English/ESOL	1.201
Exceptional Student Programs	
Support Level 1	1.341
Support Level 2	2.072
Support Level 3	3.287
Support Level 4	4.101
Support Level 5	6.860
Vocational Education, Grades 6–12	1.240

The district cost differential (DCD) adjusts the foundation level based on an average of the previous three years of the Florida Price Level Index (FPLI) as follows:

$$DCD_t^d = 0.008 \times \frac{FPLI_t^d + FPLI_{t-1}^d + FPLI_{t-2}^d}{3} + 0.2.$$

In 1998-99, DCD_t^d ranged from .9103 to 1.0751.

Districts may levy up to 0.76 mills above the foundation tax rate (0.51 mills of discretionary current operation millage and 0.25 under the discretionary tax equalization program). The Discretionary Tax Equalization (DTE) portion of the funding formula provides the difference between \$50 per $WFTE_t^d$ and what the district generates with the last 0.25 mills. Thus, DTE is given by

$$DTE_t^d = \max\left\{0, 50 \times WFTE_t^d - .00025 \times \ell_t^d W_t^d\right\}.$$

Districts with fewer than three high schools, an unweighted FTE of 20,000 or less, and unweighted FTE per high school ($SI_t^d = \max\left\{1000, \frac{FTE_t^d}{\text{\# of HS}_t^d}\right\}$) less than 7,308, are eligible for the Sparsity Supplement (SS_t^d). If the district's maximum discretionary levy (revenue generated by applying the maximum discretionary millage rate to the taxable value in the district) is above the state average, then SS_t^d is reduced by the amount by it exceeds the state average multiplied by FTE_t^d. This gives

$$SS_t^d = BSA_t \times WFTE_t^d \times \left(\frac{1101.8978}{2700+SI_t^d} - 0.01101\right) - FTE_t^d \times 0.00076 \times \max\left\{0, \frac{\ell_t^d W_t^d}{FTE_t^d} - \frac{\ell_t^s W_t^s}{FTE_t^s}\right\}.$$

In 1998-1999 the base student allocation (foundation level) was set at \$3,214.20. Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$3214.20 \times WFTE_t^d \times DCD_t^d + SS_t^d + DTE_t^d - \frac{\text{RLE Millage}_t^d}{1000} \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$3214.20 \times WFTE_t^d \times DCD_t^d + SS_t^d + DTE_t^d + \left(\tau_t^d - \frac{\text{RLE Millage}_t^d}{1000}\right) \times \ell_t^d W_t^d$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} \left(\tau_t^d - 0.000076 - \frac{\text{RLE Millage}_t^d}{1000}\right) \times \ell_t^d & \text{if } \frac{\ell_t^d W_t^d}{FTE_t^d} > \frac{\ell_t^s W_t^s}{FTE_t^s} \\ \left(\tau_t^d - \frac{\text{RLE Millage}_t^d}{1000}\right) \times \ell_t^d & \text{if } \frac{\ell_t^d W_t^d}{FTE_t^d} \leq \frac{\ell_t^s W_t^s}{FTE_t^s} \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.4 Georgia

The Quality Basic Education Act (QBE) program is a foundation program with a guaranteed yield equalization component established in GA. CODE §20-2-160. The foundation level was set at \$2,038.74 per weighted student. The local share is determined by the foundation tax rate of 5 mills or the equivalent amount of revenue. Funds are assigned based on weighted full time equivalent ($wFTE$) pupils, which are calculated by applying the weights from Table A.22 to student counts.[17] Using these weights gives

$$wFTE_t^d = 1.3286 \times FTE_t^K + 1.2432 \times FTE_t^{1-3} + 1.0197 \times FTE_t^{4-5} + 1.0242 \times FTE_t^{6-8} + FTE_t^{9-12}.$$

There are also additional program weights for students attending programs for which I do not have data, so I leave them out of my calculations.

In addition to foundation aid, the state provides guaranteed yield funding (GY) which equalizes up to 3.25 mills that are levied above the required five mills. The equalization is based on the difference between what the local district generates by

[17] These weights were put in place in 1995-1996 and I am unable to find a record of them being changed before 1998-1999. Even if they were changed, the changes are likely to be minor.

Table A.22: Georgia Pupil Weights

Program	Weight
Kindergarten program	1.3286
Primary grades program (1-3)	1.2432
Upper elementary grades program (4-5)	1.0197
Middle grades program (6-8)	1.0242
High school general education program (9-12)	1.000
High school non-vocational laboratory program (9-12)	1.2428
Vocational laboratory program (9-12)	1.3557
Program for the handicapped:	
Category I	2.3419
Category II	2.7204
Category III	3.4579

levying the 3.25 mills and what is generated by the local district at the 90th percentile in property wealth per pupil (W^{90}, including the district markup)

$$GY_t^d = \min\left\{0.00325, \max\left\{0, \tau_t^d - 0.005\right\}\right\} \times \left(\max\left\{\ell_t^d W_t^d, W_t^{90}\right\} - \ell_t^d W_t^d\right)$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$\begin{aligned} S_t^d = {} & \$2,038.74 \times wFTE_t^d - 0.005 \times \ell_t^d W_t^d \\ & + \begin{cases} 0.00325 \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ (\tau_t^d - 0.005) \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \le \tau_t^d \le \frac{8.25}{1000} \\ 0 & \text{if } \ell_t^d W_t^d \ge W_t^{90} \end{cases}, \end{aligned}$$

and total revenue is

$$R_t^d = \$2,038.74 \times wFTE_t^d + (\tau_t^d - 0.005) \times \ell_t^d W_t^d$$
$$+ \begin{cases} 0.00325 \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ (\tau_t^d - 0.005) \times \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ 0 & \text{if } \ell_t^d W_t^d \geq W_t^{90}. \end{cases}$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - 0.00825) \times \ell_t^d & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ 0 & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ \left(\tau_t^d - 0.005\right) \times \ell_t^d & \text{if } \ell_t^d W_t^d \geq W_t^{90} \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} \ell_t^d W_t^d & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \tau_t^d > \frac{8.25}{1000} \\ \ell_t^d W_t^d + \left(W_t^{90} - \ell_t^d W_t^d\right) & \text{if } \ell_t^d W_t^d < W_t^{90} \text{ and } \frac{5}{1000} \leq \tau_t^d \leq \frac{8.25}{1000} \\ \ell_t^d W_t^d & \text{if } \ell_t^d W_t^d \geq W_t^{90}. \end{cases}$$

A.3.5 Idaho

The Idaho Public School Foundation Program (PSFP) assures each district an equal dollar amount per "support unit" made up of state and local funds. Support units are a weighted student count that is intended to measure the number of teachers required to teach the population of students in the district. The distribution factor is the amount guaranteed per support unit and was \$20,758.63 in 1998-1999. The local share is the amount of property tax revenue generated by 3 mills.

Support units are determined by counts of students in kindergarten, elementary, secondary, "exceptional" education, and alternative schools. I lack data on exceptional education and alternative school counts so I omit those counts from my calculations. The divisors for converting student counts into support units are given in Table A.23. Using these values gives

$$\begin{aligned}\text{Support Units}_t^d = &\min\{U_t^K, ADA_t^K \times D_t^K\} \\ &+ \min\{U_t^{1-6}, ADA_t^{1-6} \times D_t^{1-6}\} \\ &+ \min\{U_t^{7-12}, ADA_t^{7-12} \times D_t^{7-12}\}\end{aligned}$$

where D_t^j is the divisor for group j and U_t^j are the maximum units allowed, as given in the table.

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$20,758.63 \times \text{Support Units}_t^d - 0.003 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$20,758.63 \times \text{Support Units}_t^d + (\tau_t^d - 0.003) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.003) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

Table A.23: Idaho Support Unit Weights

ADA	Attendance Divisor	Maximum Units Allowed
	K Support Units	
41 or more	40	1 or more as computed
31 to 40.99	-	1
26 to 30.99	-	0.85
21 to 25.99	-	0.75
16 to 20.99	-	0.6
8 to 15.99	-	0.5
1 to 7.99	-	count as elementary
	Elementary Support Units	
300 or more	23 (Grades 4, 5, 6)	15
	20 (Grades 1, 2, 3)	
160 to 299.99	20	8.4
110 to 159.99	19	6.8
71.1 to 109.99	16	4.7
51.7 to 71.0	15	4.0
33.6 to 51.6	13	2.8
16.6 to 33.5	12	1.4
1.0 to 16.5	N/A	1.0
	Secondary Support Units	
750 or more	18.5	47
400 to 749.99	16	28
300 to 399.99	14.5	22
200 to 299.99	13.5	17
100 to 199.99	12	9
99.99 or fewer		
Grades 7-12	-	8
Grades 9-12	-	6
Grades 7-9	-	1 per 14 ADA
Grades 7-8	-	1 per 16 ADA

A.3.6 Illinois

General State Aid (Illinois Compiled Statutes 5/18-8.05) is distributed under one of three formulas: Foundation, Alternate Method, and Flat Grant. The formula that applies to a given school district is determined by its property wealth. The state aid formula compares the district equalized assessed value (EAV) per pupil to a “state guaranteed wealth per pupil." The state guaranteed level (GL) also varies by the type of school district. For 1998-1999 the state guaranteed wealth per ADA pupil was:

- \$188,478 for elementary districts
- \$361,250 for secondary districts
- \$144,500 for unit (k-12) districts

Districts qualify for one of three formulas determined by EAV per pupil as follows:

Group Label	EAV Group	Formula
WG^1	$\frac{\ell_t^d W_t^d}{ADA_t^d} < 0.93 \times GL$	Foundation
WG^2	$0.93 \times GL < \frac{\ell_t^d W_t^d}{ADA_t^d} < 1.75 \times GL$	Alternate
WG^3	$1.75 \times GL < \frac{\ell_t^d W_t^d}{ADA_t^d}$	Flat Grant

The foundation level in 1998-1999 was \$4,225 per pupil. The local share is revenue generated by the foundation tax rate, which depends on the type of school district. The foundation tax rate is 2.3 mills for elementary districts, 1.2 mills for secondary districts, and 3 mills for unit districts. The alternative plan also uses the foundation level and local share as defined in the foundation plan, but under the following formula:

$$\$4,225 \times ADA_t^d \times \left(0.07 - \left(\frac{\text{Local Share}_t^d}{\$4,225 \times ADA_t^d} - 0.93\right) \times \frac{0.02}{0.82}\right).$$

The flat grant formula only depends on student counts as follows:

$$218 \times ADA_t^d.$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \begin{cases} \$4,225 \times ADA_t^d - (I_{\{e\}} \times 0.023 + I_{\{s\}} \times 0.012 + I_{\{u\}} \times 0.03) \times \ell_t^d W_t^d & \text{if } WG^1 \\ \$4,225 \times ADA_t^d \times \left(0.07 - \left(\frac{\text{Local Share}_t^d}{\$4{,}225 \times ADA_t^d} - 0.93\right) \times \frac{0.02}{0.82}\right) & \text{if } WG^2 \\ \$218 \times ADA_t^d & \text{if } WG^3 \end{cases},$$

where $I_{\{e\}}$ indicates elementary district, $I_{\{s\}}$ indicates secondary district, $I_{\{u\}}$ indicates unit district, and $GL^d = I_{\{e\}} \times \$188,478 + I_{\{s\}} \times \$361,250 + I_{\{u\}} \times \$144,500$. Total revenue is

$$R_t^d = \begin{cases} \$4,225 \times ADA_t^d + (\tau_t^d - (I_{\{e\}} \times 0.023 + I_{\{s\}} \times 0.012 + I_{\{u\}} \times 0.03)) \times \ell_t^d W_t^d & \text{if } WG^1 \\ \$4,225 \times ADA_t^d \times \left(0.07 - \left(\frac{\text{Local Share}_t^d}{\$4{,}225 \times ADA_t^d} - 0.93\right) \times \frac{0.02}{0.82}\right) + \tau_t^d \times \ell_t^d W_t^d & \text{if } WG^2 \\ \$218 \times ADA_t^d + \tau_t^d \times \ell_t^d W_t^d & \text{if } WG^3 \end{cases}.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - (I_{\{e\}} \times 0.023 + I_{\{s\}} \times 0.012 + I_{\{u\}} \times 0.03)) \times \ell_t^d & \text{if } WG^1 \\ (\tau_t^d - (I_{\{e\}} \times 0.023 + I_{\{s\}} \times 0.012 + I_{\{u\}} \times 0.03) \times \frac{0.02}{0.82}) \times \ell_t^d & \text{if } WG^2 \\ \tau_t^d \times \ell_t^d & \text{if } WG^3 \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.7 Iowa

Iowa has a foundation plan with an additional discretionary tier with state matching defined in statute as the School Foundation Program (1999 Code of Iowa Ch. 257).

The foundation tax rate is 5.4 mills. Spending is dictated by the district cost per pupil (DCPP) and state cost per pupil (SCPP). The foundation level is 87.5 percent of the state cost per pupil, which increases by a predetermined rate each year. The district cost per pupil is a district-specific measure that also grows at the same rate per year. Districts are also guaranteed \$300 per student in state aid, regardless of their local share dictated by the foundation tax rate.

Funds are dispersed based on weighted enrollment, which is a weighted sum of students in various programs and categories. The data for counts of students in these programs is not available historically so I use a measure of total enrollment.

Under the second tier, districts may increase their budgets by up to 10% through an "instructional support" levy. The instructional support levy is a percentage equalizing plan with the state participation at 25% for an average wealth district. Second tier funding is thus given by

$$\text{Second Tier}_t^d = \begin{cases} 0.1 \times DCPP_t^d \times ADM_t^d & \text{if } \tau_t^d > 0.0054 + \frac{0.4 \times DCPP_t^d \times ADM_t^d}{\ell_t^d W_t^d} \\ 0.25 \times (\tau_t^d - 0.0054) \times \ell_t^d W_t^d & \text{if } 0.0054 < \tau_t^d < 0.0054 + \frac{0.4 \times DCPP_t^d \times ADM_t^d}{\ell_t^d W_t^d}. \end{cases}$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \max\{300 \times ADM_t^d, 0.875 \times SCPP_t \times ADM_t^d - 0.0054 \times \ell_t^d W_t^d + \text{Second Tier}_t^d\},$$

and total revenue is

$$R_t^d = \max\{300 \times ADM_t^d, 0.875 \times SCPP_t \times ADM_t^d + \text{Second Tier}_t^d + (\tau_t^d - 0.0054) \times \ell_t^d W_t^d\}.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} 1.25 \times (\tau_t^d - 0.0054) \times \ell_t^d & \text{if } 0.0054 < \tau_t^d < 0.0054 + \frac{0.4 \times DCPP_t^d \times ADM_t^d}{\ell_t^d W_t^d} \\ (\tau_t^d - 0.0054) \times \ell_t^d & \text{if } 0.0054 + \frac{0.4 \times DCPP_t^d \times ADM_t^d}{\ell_t^d W_t^d} < \tau_t^d \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} 1.25 \times \ell_t^d W_t^d & \text{if } 0.0054 < \tau_t^d < 0.0054 + \frac{0.4 \times DCPP_t^d \times ADM_t^d}{\ell_t^d W_t^d} \\ \ell_t^d W_t^d & \text{if } 0.0054 + \frac{0.4 \times DCPP_t^d \times ADM_t^d}{\ell_t^d W_t^d} < \tau_t^d. \end{cases}$$

A.3.8 Kansas

Kansas has a foundation program that includes weights for certain programs and low enrollments as well as a discretionary tier called the local option budget (LOB) (K.S.A. §72-64, 1998). There are also additional categorical funds that I do not include in my calculations. Funds are distributed based on full time equivalent (FTE) student counts. The 1998-1999 foundation level was $3,720 per weighted FTE. Student counts are adjusted by various factors to create Weighted FTE (wFTE). The only factor for which data is available is students eligible for free or reduced price lunch, which get an additional weight of 0.08, thus I define wFTE as

$$wFTE_t^d = FTE_t^d + 0.08 \times FTE_t^{FRPL}.$$

Local effort is determined by the revenue from the foundation tax rate of 20 mills and other revenues.

The local option budget is comprised of funds raised above 20 mill foundation tax rate and state matching funds based on assessed values per pupil. LOB can be up to 25 percent of the foundation level. The state share is given by

$$\text{State Share}_t^d = \left(1 - \frac{\ell_t^d W_t^d / FTE_t^d}{P_{75}(\ell_t^d W_t^d / FTE_t^d)}\right) \times LOB_t^d$$

where $P_{75}(\ell_t^d W_t^d / FTE_t^d)$ is the 75th percentile of assessed value per pupil across districts in the state.

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$3,720 \times wFTE_t^d - 0.02 \times \ell_t^d W_t^d + \text{State Share}_t^d \times \max\{0, \tau_t^d - 0.02\} \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$3,720 \times wFTE_t^d + (\tau_t^d - 0.02) \times \ell_t^d W_t^d + \text{State Share}_t^d \times \max\{0, \tau_t^d - 0.02\} \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = 2 \times \left(1 - \frac{\ell_t^d W_t^d / FTE_t^d}{P_{75}(\ell_t^d W_t^d / FTE_t^d)}\right) \times (\tau_t^d - 0.02) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d \times \left(2 - \frac{W_t^d / FTE_t^d}{P_{75}(\ell_t^d W_t^d / FTE_t^d)}\right).$$

A.3.9 Kentucky

The Support Education Excellence in Kentucky (SEEK) funding program is a modified foundation program that includes an additional equalization component. The foundation tax rate is 3 mills and the foundation level is $2,839 per pupil in weighted average daily attendance from the previous year.[18] The equalization component allows districts to generate additional revenue up to 15% of the foundation guarantee. Local effort is equalized at 150% of the state-wide average of assessed property value per pupil. The final tier allows districts to generate up to 30% above the combination of foundation guarantee and equalized funds and is not equalized.

Funding is based on the prior year ($t-1$) number of students in weighted average daily attendance ($wADA_t^d$). Using the weights in Table A.24 gives

$$\begin{aligned} wADA_t^d = ADA_{t-1}^d + 0.15 \times ADM_{t-1}^{FRPL} + 2.35 \times ADM_{t-1}^{\text{Severe}} + 1.17 \times ADM_{t-1}^{\text{Moderate}} \\ + 0.24 \times ADM_{t-1}^{\text{Speech}} + ADM_{t-1}^{HH}. \end{aligned}$$

Table A.24: Kentucky Pupil Weighted Factors

Group	Notation	Weight
At-Risk (FRPL-Eligible)	ADM_t^{FRPL}	0.15
Severe Handicap	ADM_t^{Severe}	2.35
Moderate Handicap	ADM_t^{Moderate}	1.17
Speech Therapy	ADM_t^{Speech}	0.24
Home & Hospital	ADM_t^{HH}	1.00

State equalizing funds are given by

$$\begin{aligned} \text{Tier } 1_t^d &= wADA_t^d \\ &\times \begin{cases} 0.15 \times \$3,979.09 & \text{if } 0.003 < \tau_t^d < 0.003 + \frac{0.15 \times \$3{,}979.09}{\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}} \\ (\tau_t^d - 0.003) \times (\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}) & \text{if } \tau_t^d \geq 0.003 + \frac{0.15 \times \$3{,}979.09}{\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}}. \end{cases} \end{aligned}$$

[18] \$2,839 in 1998-99 amounts to \$3,979.09 in 2013 dollars.

Local revenue is given by

$$\tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$3,979.09 \times wADA_t^d + \text{Tier } 1_t^d - 0.003 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$3,979.09 \times wADA_t^d + \text{Tier } 1_t^d + (\tau_t^d - 0.003) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - 0.003) \times \ell_t^d & \text{if } 0.003 < \tau_t^d < 0.003 + \frac{0.15 \times \$3,979.09}{\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}} \\ (\tau_t^d - 0.003) \times \left(\ell_t^d + \frac{1.5 \ell_t^s}{ADA_t^s} - \frac{\ell_t^d}{ADA_t^d}\right) & \text{if } \tau_t^d \geq 0.003 + \frac{0.15 \times \$3,979.09}{\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}}, \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} \ell_t^d W_t^d & \text{if } 0.003 < \tau_t^d < 0.003 + \frac{0.15 \times \$3,979.09}{\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}} \\ \ell_t^d W_t^d + \left(\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}\right) & \text{if } \tau_t^d \geq 0.003 + \frac{0.15 \times \$3,979.09}{\frac{1.5 \times \ell_t^s W_t^s}{ADA_t^s} - \frac{\ell_t^d W_t^d}{ADA_t^d}}. \end{cases}$$

A.3.10 Massachusetts

Massachusetts has a foundation program referred to as Chapter 70 state aid, which was created by the Education Reform Act of 1993. The foundation amount for 1998-1999 was set at \$6,442 per pupil. A complicated formula with over 35 variables determines how this foundation amount should be adjusted based on student characteristics and the amount of required local contribution. For now, I assign the same foundation level for each district and assign a foundation tax rate of 9.4 mills.

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$6,442 \times ADM_t^d - 0.0094 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$6,442 \times ADM_t^d + (\tau_t^d - 0.0094) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.0094) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.11 Minnesota

Minnesota has a foundation program, known as the General Education Revenue program (Minnesota Statue 126C)[19]. Funds are allocated based on pupil units, which is a measure of weighted student counts. This weighted student count is

$$wADM_t^d = 0.53 \times ADM_t^K + 1.06 \times ADM_t^{1-6} + 1.3 \times ADM_t^{7-12} + ADM^D$$

where ADM_t^K is the number of kindergarten students without an IEP, ADM_t^{1-6} is the number of students in 1st through 6th grade, ADM_t^{7-12} is the number of students

[19]Full statutes are available at https://www.revisor.mn.gov/statutes/?id=126C&year=1998.

in 7th through 12th grade, and ADM_t^D is the number of pre-k and kindergarten students with a disability.

The majority of formula-based revenue is assigned through Basic Revenue, which is

$$\text{Basic Revenue}_t^d = \$3,530 \times wADM_t^d.$$

The remaining components of general education revenue are either categorical grants or based solely on student counts in particular programs. Because basic revenue accounts for about 77.7% of formula-based revenue, I estimate total General Education Revenue by dividing basic revenue by 0.777. The foundation tax rate for 1998-1999 was 0.369. If a district raises more local revenue than what is guaranteed by general education revenue, then the general education tax rate is reduced to the rate that generates exactly the guarantee. Districts can raise more than the foundation level and receive state funds to guarantee \$9,039 per weighted pupil on the first \$315 per weighted pupil above the foundation level.

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \begin{cases} \frac{\$3,530 \times wADM_t^d}{0.777} - 0.369 \times \ell_t^d W_t^d + (\tau_t^d - 0.369) \times \max\left\{0, 9039 - \frac{\ell_t^d W_t^d}{wADM_t^d}\right\} & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d \leq 315 \times wADM_t^d \\ \frac{\$3,530 \times wADM_t^d}{0.777} - 0.369 \times \ell_t^d W_t^d & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d > 315 \times wADM_t^d \end{cases}$$

and total revenue is

$$R_t^d = \begin{cases} \frac{\$3,530 \times wADM_t^d}{0.777} + (\tau_t^d - 0.369) \times \left(\ell_t^d W_t^d + \max\left\{0, 9039 - \frac{\ell_t^d W_t^d}{wADM_t^d}\right\}\right) & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d \leq 315 \times wADM_t^d \\ \frac{\$3,530 \times wADM_t^d}{0.777} + (\tau_t^d - 0.369) \times \ell_t^d W_t^d & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d > 315 \times wADM_t^d \end{cases}$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - 0.369) \times \left(\ell_t^d - \frac{\ell_t^d}{wADM_t^d}\right) & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d \leq 315 \times wADM_t^d \\ (\tau_t^d - 0.369) \times \ell_t^d & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d > 315 \times wADM_t^d \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} \left(\ell_t^d W_t^d + \max\left\{0, 9039 - \frac{\ell_t^d W_t^d}{wADM_t^d}\right\}\right) & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d \leq 315 \times wADM_t^d \\ \ell_t^d W_t^d & \text{if } (\tau_t^d - 0.369) \times \ell_t^d W_t^d > 315 \times wADM_t^d \end{cases}.$$

A.3.12 Mississippi

State aid to school districts in Mississippi is described by the Mississippi Adequate Education Program (Miss. Stat. §37-151-7) operates like a guaranteed yield plan. The Base Student Cost (BSC) is calculated based on the funding of schools with an adequate proficiency rating as a baseline. In 1998-1999, the BSC was $2,787. The guaranteed funding for each district, known as the Adequate Education Program Cost (AEPC), is

$$AEPC_t^d = BSC_t \times (ADM_t^d + 0.05 \times \text{Free-Lunch}_t^d) + \text{Add-ons}_t^d$$

where Free-Lunch_t^d is the number of students participating in the Free Lunch Program and Add-ons_t^d is the sum of 8 categorical grants for transportation, vocational/technical education, special education, gifted education, alternative school programs, extended school year programs, university-based programs, and bus drive training programs. To participate in the MAEP and receive state aid, districts must provide revenue from levying 28 mills of local property tax or 27 percent of the $AEPC_t^d$, whichever is less.

The add-on grants are unrelated to district wealth and tax rates, so I do not include them in my formulas. Local revenue is

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$2,787 \times (ADM_t^d + 0.05 \times \text{Free-Lunch}_t^d) - \min\left\{0.028 \times \ell_t^d W_t^d, 0.27 \times \$2,787 \times ADM_t^d\right\},$$

and total revenue is

$$R_t^d = \tau_t^d \times \ell_t^d W_t^d + \$2,787 \times (ADM_t^d + 0.05 \times \text{Free-Lunch}_t^d) - \min\left\{0.028 \times \ell_t^d W_t^d, 0.27 \times \$2,787 \times ADM_t^d\right\}.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.028) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.13 Nevada

The Nevada Plan is a minimum foundation program that provides guaranteed funding per weighted pupil (NEV. REV. STATE. § 387.121). The amount of the guarantee, called the basic support guarantee, is adjusted for each district based on a number of factors. The average basic support guarantee was \$3,802 in 1999-2000 (the earliest year available). State financial aid to school districts equals the difference between school district basic support guarantee and local available funds produced by mandatory taxes. Nevada has a 2.25% local sales tax that funds about half of the total guarantee. I treat this as additional state revenue transferred to districts. The local share of property tax accounts for less than 10 percent of the guarantee, but districts raise twice as much money as that local share as part of the mandatory 0.0075 levy.

The state also provides a set amount of special education funding each year. For example, in 1998-1999, 2,088 units were funded by the Legislature at $28,248 per unit for a total appropriation of $58,981,824. These per-unit funds are not enough to cover the full cost of the special education program unit and there may be more units in the state than the total appropriation. Districts are required to have one special education program unit per 16 students with an IEP.

Local revenue is given by

$$L_t^d = 0.0075 \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$3,802 \times \left(0.6 \times ADM_t^k + ADM_t^{1-12}\right) + \$28,248 \times \frac{IEP_t^d}{16} - 0.0025 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$3,802 \times \left(0.6 \times ADM_t^k + ADM_t^{1-12}\right) + \$28,248 \times \frac{IEP_t^d}{16} + 0.005 \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = 0.005 \times \ell_t^d$$

and the tax price does not apply in this situation because districts are not allowed to increase their tax rate.

A.3.14 New Hampshire

The New Hampshire legislature adopted a new school funding formula, called the Adequate Education Funding Plan (AEFP), for the 1999-2000 school year in response to court challenges to their current system, which primarily relied on local funding for education. The AEFP provides \$4,220 per pupil in funding, about half of which is generated by a 6.6 mill statewide property tax. Funds are assigned based on the weighted number of pupils, such that

$$wADM_t^d = ADM_t^{k-6} + 1.2 \times ADM_t^{7-12} + ADM_t^{IEP} + \begin{cases} 0 & \text{if } \frac{FRPL_t^d}{ADM_t^d} < 0.12 \\ 0.5 \times FRPL_t^d & \text{if } 0.12 \leq \frac{FRPL_t^d}{ADM_t^d} \leq 0.24 \\ FRPL_t^d & \text{if } 0.24 < \frac{FRPL_t^d}{ADM_t^d} \end{cases}$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$4,220 \times wADM_t^d - 0.066 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$4,220 \times wADM_t^d + (\tau_t^d - 0.066) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.066) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.15 New Jersey

The school finance plan in New Jersey as of 1998-1999 was established by the "Comprehensive Educational Improvement and Financing Act of 1996," (CEIFA) (N.J. STAT. ANN. §18a: 1–1 et seq. CEIFA). Due to a New Jersey Supreme Court decision, Abbott v. Burke 575 A.2d 359 (N.J. 1990), CEIFA requires the state make funding in 28 low-income, urban school districts equivalent to spending the most affluent districts. CEIFA is a minimum foundation grant program with 24 additional aid programs.

The foundation amount per pupil is called the T&E (Thorough & Efficient) amount and the total foundation budget is called the T&E budget. The T&E budget is the level of spending determined by the state to be necessary to support a quality education. The T&E budget is a district's weighted enrollment times the T&E amount. That is,

$$\text{T\&E Budget}_t^d = \text{Weighted Pupils}_t^d \times \text{T\&E Amount}_t$$

Each year the T&E amount is increased by an amount equal to the annual percentage increase in the Consumer Price Index (CPI). For the 1998-1999 school year, the T&E amount is \$6,899. There is an additional "T&E flexible amount," which give a range around the T&E amount that is also considered acceptable. The T&E flexible amount was \$336 for 1997-1998, so adjusted by the CPI makes it $336*(1+.0220859) \approx 343.42$ for 1998-1999.

Using the numbers from Table A.25, we get

$$\text{Weighted Pupils}_t^d = 0.5 \times ADM_t^k + ADM_t^{1-5} + 1.12 \times ADM_t^{6-8} + 1.2 \times ADM_t^{9-12}$$

Table A.25: Per Pupil Weighting Factors

	Weight	T&E Amount	T&E Range
Kindergarten	.50	\$3,450	\$3,278-\$3,623
Elementary School	1.0	\$6,899	\$6,544-\$7,244
Middle School	1.12	\$7,727	\$7,341-\$8,113
High School	1.2	\$8,279	\$7,865-\$8,693

The local share is determined by three factors: (a) the total amount of aid to be allocated through the CCSA formula statewide; (b) the district's income; and, (3) the district's property wealth. Specifically,

$$\text{Local Share}_t^d = \text{T\&E Budget}_t^d \times \left(\frac{WRT_t \times W_t^d + IRT_t \times I_t^d}{2}\right)$$

where WRT is the wealth ratio and IRT is the income ratio. The wealth ratio is given by

$$WRT_t = \frac{\text{T\&E Budget}_t^d}{W_t^s},$$

and in 2008-2009, the WRT was 0.0092690802. The income ratio is based on

$$IRT_t = \frac{\text{T\&E Budget}_t^d}{I_t^s},$$

which was 0.04546684 in 2008-2009.

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \text{Weighted Pupils}_t^d \times \text{T\&E Amount}_t \times (1 - WRT_t \times \ell_t W_t^d),$$

and total revenue is

$$R_t^d = \tau_t^d \times \ell_t^d W_t^d + \text{Weighted Pupils}_t^d \times \text{T\&E Amount}_t \times (1 - WRT_t \times \ell_t W_t^d).$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \ell_t^d(\tau_t^d - WRT_t \times \text{T\&E Amount}_t)$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.16 New Mexico

School funding in New Mexico is a foundation plan determined by the New Mexico Public School Finance Act of 1974. Funds are distributed based on weighted student counts called program units. Total program units are given by

$$\begin{aligned}\text{Total Program Units}_t^d = &\ 1.44 \times FTE_t^k + 1.2 \times FTE_t^1 + 1.18 \times FTE_t^{2-3} \\ &+ 1.045 \times FTE_t^{4-6} + 1.25 \times FTE_t^{7-12} \\ &+ SpecEd_t^d + Bilingual_t^d.\end{aligned}$$

There is not enough data available to calculate the $SpecEd_t^d$ and $Bilingual_t^d$ portions separately, so I omit them in my simulated funding.

The foundation level in 1998-1999 was $2,344.09 per weighted student. The required minimum local levy is 0.5 mills.

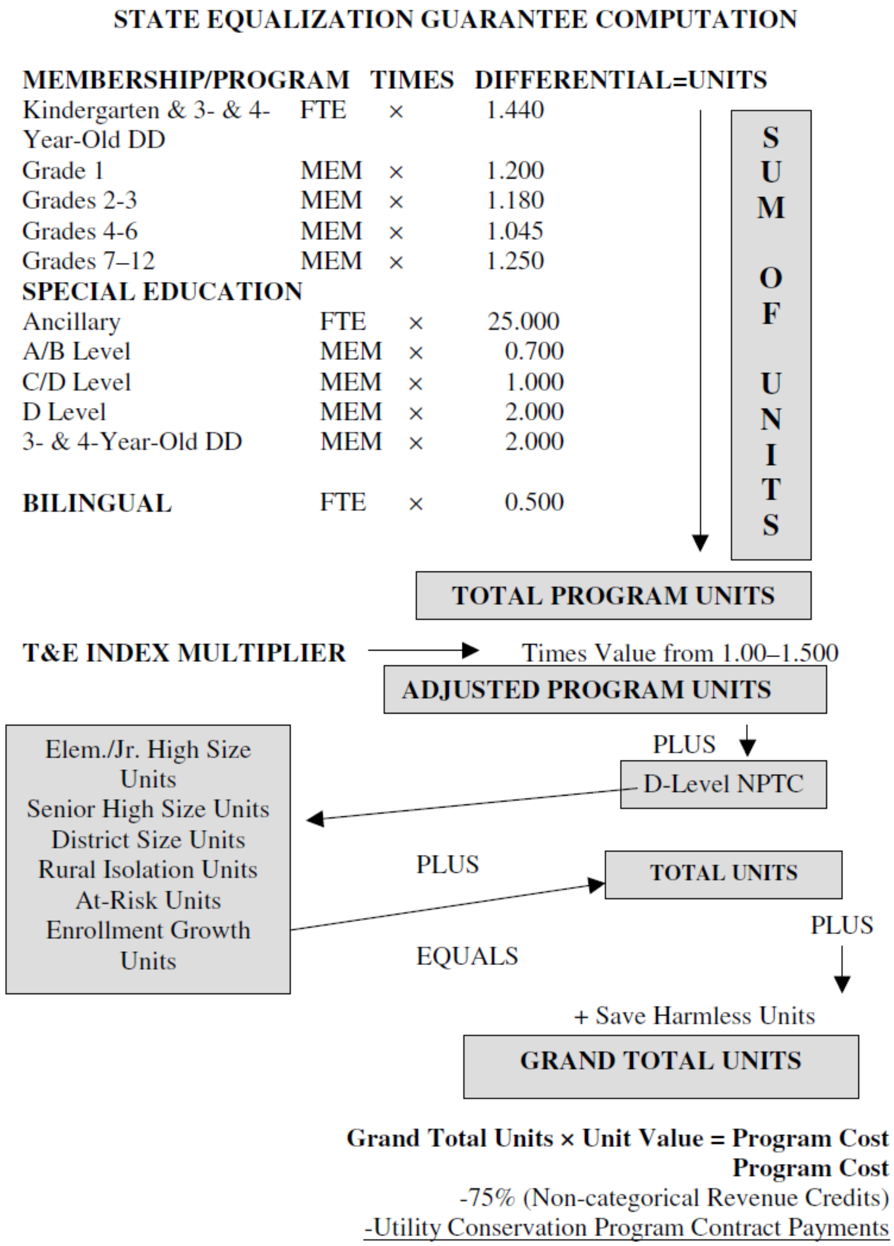
Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$2,344.09 \times \text{Total Program Units}_t^d - 0.0005 \times \ell_t^d W_t^d,$$

Figure A.7: New Mexico Funding Formula



Source: U.S. Department of Education National Center for Education Statistics (2001)

and total revenue is

$$R_t^d = \$2,344.09 \times \text{Total Program Units}_t^d + (\tau_t^d - 0.0005) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.0005) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.17 New York

State aid for education in New York is distributed as Basic Operating Aid (BOA), Extraordinary Needs Aid, Growth Aid, Tax Effort Aid, Tax Equalization Aid, and Transition Adjustment (N.Y. CODE §3602 (12)). BOA accounts for about half of state funding and most funds are equalized.

Funds are assigned based on weighted pupils, called Total Aidable Pupil Units (TAPU). The weights depend on the number of students in certain grades and programs as described in Table A.26. Data is unavailable for summer school and dual enrollment, so I calculate TAPU as

$$TAPU_t^d = 0.5 \times ADM_t^{k-halfday} + ADM_t^{k-6} + 1.25 \times ADM_t^{7-12} + 0.25 \times ADM_t^{IEP}$$

The primary source of state funds comes in the form of Basic Operating Aid (BOA), which is

$$BOA_t^d = \begin{cases} TAPU_t^d \times 400 & \text{if Aid Per Pupil}_t^d \leq 400 \\ TAPU_t^d \times \text{Aid Per Pupil}_t^d & \text{if Aid Per Pupil}_t^d > 400 \end{cases}$$

Table A.26: Pupil Weights for Calculating TAPU

Grade	Weight
1/2 Day K	0.50
Full Day K-6 (excluding Special Education)	1.00
Full Day K-6 Special Education	1.25
7-12 (excluding Special Education)	1.25
7-12 Special Education	1.50
Summer School	0.12
Dual Enrollment	fraction of day in public school programs

Aid Per Pupil$_t^d$ is the result of applying a number of district-level adjustment factors to the basic foundation level, called the Approved Operating Expenses (AOE) which are established by the legislature each year. Specifically,

$$\text{Aid Per Pupil}_t^d = OAR_t^d \times (\$3,900 + \text{Ceiling Adjustment}_t^d)$$

where

$$OAR_t^d = \begin{cases} \min\left\{0.9, 1.37 - (1.23 - CWR_t^d)\right\} & \text{if } CWR_t^d < 0.627 \\ 1.00 - (0.64 \times CWR_t^d) & \text{if } 0.627 \leq CWR_t^d < 0.8 \\ 0.80 - (0.39 \times CWR_t^d) & \text{if } 0.8 \leq CWR_t^d < 1.706 \\ \max\left\{0.0, 0.51 - (0.22 \times CWR_t^d)\right\} & \text{if } CWR_t^d \geq 1.706 \end{cases}$$

and

$$\text{Ceiling Adjustment}_t^d = \frac{0.075}{CWR_t^d} \times \min\left\{\$8,000, \frac{AOE_{t-2}^d}{TAPU_{t-2}^d} - \$3,900\right\}.$$

CWR is the Combined Wealth Ratio and adjusts for the district's property wealth and aggregate income.[20]

[20]Specifically, $CWR_t^d = 0.5\left(\frac{W_{t-3}^d}{W_{t-3}^s}\frac{TAPU_{t-2}^s}{TAPU_{t-2}^d} + \frac{IPP_t^d}{IPP_t^s}\right)$ where $IPP_t^d = \frac{\text{Income}_{t-3}^d}{TAPU_{t-2}^d}$.

There are other grants for state aid but most do not vary with property wealth and are irrelevant for the variation in my instrument. An exception is Tax Equalization Aid, which is the primary foundation adjustment. Specifically,

$$\text{Tax Equalization}_t^d = \left(\min\left\{8000, \frac{AOE_t^d - BOA_t^d}{TAPU_t^d}\right\} - 0.0195 \times \frac{\ell_t^d W_t^d}{TAPU_t^d}\right) \times TAPU_t^d,$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = BOA_t^d + \text{Tax Equalization}_t^d,$$

and total revenue is

$$R_t^d = \tau_t^d \times \ell_t^d W_t^d + BOA_t^d + \text{Tax Equalization}_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.0195) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.18 North Carolina

School funding in North Carolina is unique among the states. The state provides a level of funds to each district based on weighted student counts, which they use to determine the cost of the number of teachers needed to teach those students.

Districts can choose to levy additional property taxes to increase spending, but are under no obligation to do so and local levies have no impact on state aid.

The state funds PSAT testing in the amount of 2.69 for each 8th and 9th grade student; instructional materials, supplies, instructional equipment, and testing support at \$40.29 per student; and textbooks at the rate of \$46.77 per student. The

Table A.27: Number of Students per Alloted Teacher

Grades	Number of Students
K-2	20
3	22.23
4-6	22
7-8	21
9	24.5
10-12	26.64

base teacher allotment is a weighted number of students based on the weights in Table A.27 and rounded to the nearest one-half position. This results in a teacher allotment of

$$\text{Teacher Allotment}_t^d = \frac{ADM_t^{k-2}}{20} + \frac{ADM_t^3}{22.23} + \frac{ADM_t^{4-6}}{22} + \frac{ADM_t^{7-8}}{21} + \frac{ADM_t^9}{24.5} + \frac{ADM_t^{10-12}}{26.64}$$

Each county is given one additional teacher allotment for a math/science/computer teacher regardless of student counts. School districts are also allotted one position per 200.10 ADM for instructional support. Teacher assistants are allotted at \$749.64 per ADM in grades K-3. One principal is allowed per school and assistant principals are allowed as 1 month per 76.12 ADM.

The total salary allocation for each district is the number of teachers times the salary allocated to each type of teacher. Base teacher pay is \$38,065 plus \$3,307 in benefits, instructional support positions receive \$45,973 including benefits, principals receive \$46,940, and assistant principals receive \$46,125. This gives a total salary allocation of

$$\text{Salaries}_t^d = 41,372 \times (\text{Teacher Allotment}_t^d + 1) + 45,973 \times \frac{ADM_t^d}{200.1}$$
$$+ 46,940 \times \text{Number of Schools}_t^d + 46,125 \times \frac{ADM_t^d}{76.12}\frac{1}{12}$$

There are two additional grants to cover retirement and social security benefits that is an additional 11.07% of the salary allocation, or Additional $\text{Benefits}_t^d = 0.1107 \times \text{Salaries}_t^d$.

Combining the above information gives local revenue as

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$2.69 \times ADM_t^{8-9} + \$40.29 \times ADM_t^d + \$46.77 \times ADM_t^d + \$749.64 \times ADM_t^{k-3} + 1.1107 \times Salaries_t^d,$$

and total revenue is

$$R_t^d = \tau_t^d \times \ell_t^d W_t^d + \$2.69 \times ADM_t^{8-9} + \$87.06 \times ADM_t^d + \$749.64 \times ADM_t^{k-3} + 1.1107 \times Salaries_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \tau_t^d \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.19 North Dakota

North Dakota has an equalized foundation formula (N.D. CENT. CODE §15-40.1), which is distributed based on weighted student counts, called weighted pupil units (WPU). In 1998-1999, the foundation level was $2,032 per WPU and the foundation tax levy was 32 mills. There is no recapture provision so state aid is the maximum of the formula calculation and zero.

Table A.28: Weights for Calculating Weighted Pupil Units

Category (Grade level & size)	Statutory Weighting	Weighting Factor Applied in 1998-1999
Approved preschool	1.010	1.2924
Kindergarten (all districts)	0.500	0.5720
Rural elementary (1-8)	1.280	1.3198
Grades 1-6 (<100 ADM)	1.090	1.2012
Grades 1-6 (100-999)	0.905	0.9477
Grades 1-6 (1,000+)	0.950	0.9706
Grades 7-8 (all districts)	1.010	0.9832
Grades 9-12 (<75 ADM)	1.625	1.4905
Grades 9-12 (75-149)	1.335	1.1981
Grades 9-12 (150-549)	1.240	1.0917
Grades 9-12 (550+)	1.140	1.0473

Source: U.S. Department of Education National Center for Education Statistics (2001)

Applying the student weights described in Table A.28 gives

$$WPU_t^d = 1.2924 \times ADM_t^{pk} + 0.572 \times ADM_t^k$$

$$+ (I_{<100} \times 1.2012 + I_{100 \text{ to } 999} \times 0.9477 + I_{\geq 1000} \times 0.9706) \times ADM_t^{1-6}$$

$$+ 0.9832 \times ADM_t^{7-8}$$

$$+ (I_{<75} \times 1.4905 + I_{75 \text{ to } 149} \times 1.1981 + I_{150 \text{ to } 549} \times 1.0917 + I_{>550} \times 1.0473) \times ADM_t^{9-12}$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$2.302 \times WPU_t^d - 0.032 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$2.302 \times WPU_t^d + (\tau_t^d - 0.032) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \tau_t^d W_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.20 Ohio

The main school funding program in Ohio is called the School Foundation Funding Program (Ohio Revised Code §3317) and provides a foundation level funded by state and local revenue, and additional categorical grants from the state to school districts. The foundation amount was \$3,851 in 1998-1999, which is further adjusted by a Cost of Doing Business (CODB) factor that captures regional differences in the cost of living. The foundation tax rate is 23 mills. Foundation funds are distributed based on weighted student counts with the following weights: 0.5 for kindergarten; 1.0 for grades 1-12; 0.25 for vocational education pupils who receive services from other educational units; and three major categories of special education weighting: The mildest category gives an additional .22 weighting, the next category gets an additional 3.01 weighting, and the most severe category gets an additional 3.01 weighting but allows for the additional state aid to subsidize more expensive individual educational program costs. I do not have counts of vocational education pupils or the severity of conditions for students with an IEP in the data so I do not include the vocational education weighting and assign each student with an IEP the minimum weight of 0.22. Using these weighting factors gives a weighted average daily membership (wADM) of

$$wADM_t^d = 0.5 \times ADM_t^k + ADM_t^{1-12} + 0.22 \times ADM_t^{IEP}.$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$3,851 \times CODB_t^d \times wADM_t^d - 0.023 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \$3,851 \times CODB_t^d \times wADM_t^d + (\tau_t^d - 0.023) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.023) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.21 Oklahoma

Oklahoma has a two-tiered funding program with a foundation amount as well as a guaranteed yield portion. The foundation tax rate is established in the Oklahoma State Constitution to be 15 mills (Oklahoma Constitution Article X §9(c)). Districts also have a responsibility for 75% of the revenue collected by a countywide tax of 4 mills I use 18 mills as the foundation tax rate, although the additional 3 mills from the countywide tax will be weighted based on the fraction of county property wealth that is in the school district, which I will not be capturing. The base foundation amount was set at $1,239 per weighted pupil. Weights for calculating weighted average daily

Table A.29: Oklahoma Pupil Weights

Group	Weight
Half-day early childhood programs	0.7
Full-day early childhood programs	1.3
Kindergarten	1.3
First and second grade	1.351
Third grade	1.051
Fourth through sixth grade	1.0
Seventh through twelfth grade	1.2
Out-of-home placement	1.5

Source: Oklahoma Statute Title 70 §18-201.1

membership are given in Table A.29. Data for half-day early childhood programs and out-of-home placements are unavailable so I calculate weighted students as

$$wADM_t^d = 1.3ADM_t^k + 1.351ADM_t^{1-2} + 1.051ADM_t^3 + ADM_t^{4-6} + 1.2ADM_t^{7-12}.$$

The second tier of state aid is a guaranteed yield program called Salary Incentive Aid. As of 1998-1999, the state guaranteed districts $59.93 per weighted student for every mill levied above the 18 mill minimum requirement. There is a constitutional cap of a maximum of 20 mills above the minimum requirement. The Salary Incentive Aid can thus be written as

$$\text{Salary Incentive Aid}_t^d = \max\{0, 59.93 \times wADM_t^d - (\tau_t^d - 0.018) \times \ell_t^d W_t^d\}$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \$1,239 \times wADM_t^d - 0.018 \times \ell_t^d W_t^d + \text{Salary Incentive Aid}_t^d,$$

and total revenue is

$$R_t^d = \$1,239 \times wADM_t^d + (\tau_t^d - 0.018) \times \ell_t^d W_t^d + \text{Salary Incentive Aid}_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} 0 & \text{if } W_t^d \leq \frac{59.93 \times wADM_t^d}{(\tau_t^d - 0.018) \times \ell_t^d} \\ (\tau_t^d - 0.018) \times \ell_t^d & \text{if } W_t^d > \frac{59.93 \times wADM_t^d}{(\tau_t^d - 0.018) \times \ell_t^d} \end{cases}$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} \$59.93 & \text{if } \tau_t^d \leq \frac{59.93 \times wADM_t^d}{\ell_t^d W_t^d} + 0.018 \\ \ell_t^d W_t^d & \text{if } \tau_t^d > \frac{59.93 \times wADM_t^d}{\ell_t^d W_t^d} + 0.018. \end{cases}$$

A.3.22 Oregon

Oregon uses a foundation program called the State School Fund (OR Rev. Stat. Ch. 327). In 1998-1999, the foundation level was $4,500 per weighted pupil and the foundation tax rate was 5 mills. The foundation amount is adjusted by the relative experience level of teachers in the district, compared to the rest of the state. The earliest information available for teacher experience is for the 2005-2006 school year and I assign these values for each year. Specifically the foundation level for each district is

$$\text{Foundation}_t^d = \$4,500 + 25 \times \text{Teacher Experience Adjustment}_t^d$$

where

$$\text{Teacher Experience Adjustment}_t^d = \text{Teacher Experience}_t^d - \text{Teacher Experience}_t^s.$$

Each pupil receives a weight of 1 and students receive additional weights: 1 for each student in special education; 0.5 for student with English as a second language; 0.2 for students attending a union high school district; -0.1 for students in an elementary school district; and 0.25 for students in poverty, students in foster homes, and students in state facilities. Data is only available for the number of students in each grade, in special education, or eligible for free/reduced-price lunch. Thus, leaving out the weights based on unavailable information, weighted pupils are given by

$$wADM_t^d = ADM_t^d + ADM_t^{SpecEd} + 0.25ADM_t^{FRPL}.$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \text{Foundation}_t^d \times wADM_t^d - 0.005 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \text{Foundation}_t^d \times wADM_t^d + (\tau_t^d - 0.005) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.005) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

A.3.23 Texas

School funding in Texas is given by a two-tiered scheme called the Foundation School Program (Texas Education Code §42). The first tier is a foundation program with a 8.6 mill foundation tax rate and a base foundation level of $2,396. The second tier guaranteed $21 in revenue per weighted pupil per 0.1 mills from 8.6 to 15 mills.

The base foundation level is adjusted by several district-specific measures that account for differences in the cost of living and costs associated with educating students in small or rural school districts. The basic allotment (BA_t^d) was $2,396 in 1998-1999. The adjusted basic allotment (ABA_t^d) takes into account the Cost of Education Index (CEI_t^d) as follows:

$$ABA_t^d = \$2,396 \times (((CEI_t^d - 1) \times 0.71) + 1).$$

The small district adjustment (SDA_t^d) applies to districts with fewer than 1,600 students and is given by

$$SDA_t^d = \begin{cases} (1 + (1600 - ADM_t^d) \times 0.00025) \times ABA_t^d & \text{if Square Miles}_t^d < 300 \\ (1 + (1600 - ADM_t^d) \times 0.0004) \times ABA_t^d & \text{if Square Miles}_t^d > 300. \end{cases}$$

The mid-sized district adjustment (MDA_t^d) applies to districts with fewer than 5,000 students and is given by

$$MDA_t^d = (1 + (5000 - ADM_t^d) \times 0.000025) \times ABA_t^d.$$

The adjusted allotment (AA_t^d) is then defined as the maximum of the adjusted basic allotment, small district adjustment, and mid-sized district adjustment.

Table A.30 gives the weights used for each program. The non-special education elements can be summarized as

$$\begin{aligned} \text{Foundation}_t^d = AA_t^d \times (&ADM_t^d + 1.35 \times ADM_t^{CATE} + 0.12 \times ADM_t^{GT} \\ &+ 0.1 \times (ADM_t^{ESL} + ADM_t^{PEG}) + 0.2 \times ADM_t^{CE} + 2.41 \times ADM_t^{P}) \end{aligned}$$

and the special education elements as

$$\begin{aligned} \text{SpecEd}_t^d = (&5 \times (ADM_t^{C0} + \times ADM_t^{C1}) \\ &+ 3 \times (ADM_t^{C2} + ADM_t^{C41,C42} + ADM_t^{SMM}) \\ &+ 2.7 \times ADM_t^{C91-C98} + 2.3 \times ADM_t^{C8} + 2.8 \times ADM_t^{C30} \\ &+ 1.7 \times ADM_t^{NPC} + 4 \times ADM_t^{C81-C89}) \times AA_t^d. \end{aligned}$$

There are two additional categorical grants for New Instructional Facilities, given by $NIF_t^d = \$250 \times ADM_t^{NIF}$, and Transportation, given by T_t^d. Total Tier I funding is then given by

$$\text{Tier I}_t^d = \text{Foundation}_t^d + \text{SpecEd}_t^d + NIF_t^d + T_t^d.$$

Table A.30: Tier I Program Weights

Program	Weight
Regular Block Grant (RBG)	1.00
Career & Technology Allotment (CATE)	1.35
Gifted & Talented Allotment (GT)	0.12
Bilingual/ESL Allotment (ESL)	0.1
Public Education Grant (PEG)	0.1
Compensatory Education Allotment (CE)	0.2
Self-contained, Pregnant (P)	2.41
Special Education	
Homebound (Code 01)	5
Hospital Class (Code 02)	3
Speech Therapy (Code 00)	5
Resource Room (Codes 41 & 42)	3
Self-Contained Severe/Moderate/Mild (SMM)	3
Off-Home Campus (Code 91-98)	2.7
Vocational Adjustment Class (Code 08)	2.3
State Schools (Code 30)	2.8
Non-Pubic Contracts (NPC)	1.7
Residential Care and Treatment (Codes 81-89)	4

Source: U.S. Department of Education National Center for Education Statistics (2001)

Tier 2 funding provides a guaranteed return to each unit of property tax regardless of district property wealth. The guaranteed return is based on Weighted Average Daily Attendance ($WADA_t^d$), which is given by

$$WADA_t^d = \frac{\text{Tier } 1_t^d - T_t^d - 0.5(ABA_t^d - \$2,396)}{\$2,396}.$$

Specifically, districts are guaranteed revenue as if they had \$210,000 per $WADA_t^d$ in property wealth on any millage between 8.6 and 15. Additionally, if the district has more than \$280,000 per $WADA_t^d$ in property wealth, then the state recaptures the amount above \$280,000 per $WADA_t^d$ on those mills. Thus, Tier 2_t^d funding is given by

$$\text{Tier } 2_t^d = \begin{cases} \min\{0.015, \tau_t^d - 0.0086\} \times \left(\$210,000 - \frac{\ell_t^d W_t^d}{WADA_t^d}\right), & \text{if } W_t^d \leq \frac{1}{\ell_t^d}\$210,000 \times WADA_t^d \\ \min\{0.015, \tau_t^d - 0.0086\} \times \left(\$280,000 - \frac{\ell_t^d W_t^d}{WADA_t^d}\right), & \text{if } W_t^d \geq \frac{1}{\ell_t^d}\$280,000 \times WADA_t^d \\ 0, & \text{otherwise} \end{cases}$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = \text{Tier } 1_t^d + \text{Tier } 2_t^d - 0.0086 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = \text{Tier } 1_t^d + \text{Tier } 2_t^d + (\tau_t^d - 0.0086) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = \begin{cases} (\tau_t^d - 0.0086) \times \ell_t^d (1 - \frac{1}{WADA_t^d}), & \tau_t^d < 0.015 \\ (\tau_t^d - 0.0086) \times \ell_t^d - \frac{0.0064\ell_t^d}{WADA_t^d}, & \tau_t^d > 0.015 \end{cases}$$

other than the group whose property wealth is $\frac{1}{\ell_t^d}\$210,000 \times WADA_t^d \leq W_t^d \leq \frac{1}{\ell_t^d}\$280,000 \times WADA_t^d$, whose wealth price is simply $(\tau_t^d - 0.0086) \times \ell_t^d$. The tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \begin{cases} \$210,000 + \ell_t^d W_t^d (1 - \frac{1}{WADA_t^d}), & \text{if } W_t^d \leq \frac{1}{\ell_t^d}\$210,000 \times WADA_t^d \\ \$280,000 + \ell_t^d W_t^d (1 - \frac{1}{WADA_t^d}), & \text{if } W_t^d \geq \frac{1}{\ell_t^d}\$280,000 \times WADA_t^d \\ \ell_t^d W_t^d, & \text{otherwise.} \end{cases}$$

A.3.24 Washington

Washington state provides full funding for basic education (Washington Revised Code §§28A.150 and 28A.510). Funds are assigned based on the number of teachers deemed necessary to provide education for the particular makeup of the school district. The makeup of the school district is determined by the full-time equivalent (FTE) counts of students in different grades and programs. There are a number of different allocations for types of teachers based on weighted enrollment and an allocation per teacher unit.

The number of basic education certificated instructional staff formula units (CISFUs) generated per 1,000 FTE students depends on grade level and program. For the grade 4-12 regular education program, districts get 1 staff unit per 21.74 students. For grades K-3, districts get funding based on their actual staff to student ratio, with a maximum of 1 staff unit per 18.42 students (for simplicity, I assign all districts the maximum). School districts get 1 staff per 19.5 students in secondary vocational programs and 1 staff units per 18.2 FTE students enrolled in skill center programs.

Certified instructional staff formula units are given by

$$CISFU_t^d = \frac{FTE_t^{4-12}}{21.74} + \frac{FTE_t^{k-3}}{18.42},$$

Certified administrative staff formula units

$$CASFU_t^d = \frac{FTE_t^d}{250},$$

and classified staff formula units are given by

$$CSFU_t^d = \frac{16.67}{1000} \times FTE_t^d.$$

These three formula unit measures are then multiplied by a factor that takes into account the salary for each teacher unit. This requires data on the education level and tenure of all teachers in the data, which I do not have access to for 1999. Instead, I use the average salary for teachers in Washington in 1999, which is \$54,231.43 in 2013 dollars (38,693 in 1999 dollars). Thus, the salary support for the basic education program would be

$$\text{Salaries}_t^d = \$54,231.43 \times (CISFU_t^d + CASFU_t^d + CSFU_t^d)$$

Data is unavailable for the number of students in secondary vocational programs and skill center programs. However, I know that, in 1999, combined spending on vocational education and skill center programs was 7.8 percent of basic education, so I add 7.8 percent of my calculation of basic education funding. In fact, there are several other categories (special education, transportation, food services, etc.) that I do not have data to calculate for each district, but I know all these programs (including the 7.8 in vocational/skills) are 54.1 percent the size of basic education, so I add this into the calculation to get in the right ballpark.

In addition to salary support, the state provides:

1. \$5,646.98 (\$4,029 in 1999 dollars) per certificated and classified staff unit for insurance,
2. \$11,286.94 (\$8,053 in 1999 dollars) per basic education certificated staff unit for non-employee related costs (e.g., books, supplies, heat);
3. \$27,716.29 (\$19,775 in 1999 dollars) per secondary vocational staff formula unit for non-employee related costs,
4. \$21,505.88 (\$15,344 in 1999 dollars) per skills center certificated staff formula unit for non-employee related costs; and
5. \$511.97 (\$365.28 in 1999 dollars) per certificated instructional staff formula unit for substitute teachers.

Items 3 and 4 will be accounted for in the additional 54.1 percent of basic aid. The other categories for insurance benefits, non-employee related costs, and substitute teachers will be

$$\begin{aligned}
\text{Insurance}_t^d &= \$5,646.98 \times (CISFU_t^d + CASFU_t^d + CSFU_t^d) \\
\text{Non-Employee Costs}_t^d &= \$11,286.94 \times (CISFU_t^d + CASFU_t^d) \\
\text{Subsitutes}_t^d &= \$511.97 \times CISFU_t^d
\end{aligned}$$

Local revenue is given by

$$L_t^d = \tau_t^d \times \ell_t^d W_t^d,$$

state revenue is

$$S_t^d = 1.541 \times (\text{Salaries}_t^d + \text{Insurance}_t^d + \text{Non-Employee Costs}_t^d + \text{Subsitutes}_t^d) - 0.01 \times \ell_t^d W_t^d,$$

and total revenue is

$$R_t^d = 1.541 \times (\text{Salaries}_t^d + \text{Insurance}_t^d + \text{Non-Employee Costs}_t^d + \text{Subsitutes}_t^d) + (\tau_t^d - 0.01) \times \ell_t^d W_t^d.$$

Thus, the wealth price is

$$\frac{\partial R_t^d}{\partial W_t^d} = (\tau_t^d - 0.01) \times \ell_t^d$$

and the tax price is

$$\frac{\partial R_t^d}{\partial \tau_t^d} = \ell_t^d W_t^d.$$

APPENDIX B

THE INTERGENERATIONAL IMPACT OF CIGARETTE TAXES ON SMOKING INITIATION – APPENDIX

Table B.1: Discrete-time Hazard Model of Smoking Initiation, Alternative Start Ages

	(1) Age=6	(2) Age=7	(3) Age=8	(4) Age=9	(5) Age=10
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)					
Cigarette Tax in Childhood	0.322**	0.360**	0.498**	0.514**	0.543**
	(0.064)	(0.068)	(0.080)	(0.080)	(0.079)
Current Cigarette Tax	1.082	1.078	1.062	1.061	1.058
	(0.058)	(0.056)	(0.053)	(0.056)	(0.058)
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)					
Cigarette Tax in Childhood	-0.169**	-0.160**	-0.125**	-0.122**	-0.114**
	(0.016)	(0.017)	(0.020)	(0.020)	(0.020)
Current Cigarette Tax	0.020	0.020	0.015	0.015	0.014
	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Mean Smoking Initiation Hazard	0.046	0.050	0.054	0.059	0.064
Individuals	7,907	8,102	8,228	8,151	8,059
Observations	105,230	97,377	89,289	81,071	72,943

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Table B.2: Discrete-time Hazard Model of Smoking Initiation with Alternative Functional Form Assumptions

	(1) Complementary Log-log	(2) Logit	(3) Probit
	A. Exponentiated Coefficients ($H_0 : e^{\beta} = 1$)		
Cigarette Tax in Childhood	0.498**	0.492**	
	(0.080)	(0.082)	
Current Cigarette Tax	1.062	1.068	
	(0.053)	(0.055)	
	B. Marginal Effects		
Cigarette Tax in Childhood	-0.125**	-0.158**	-0.132**
	(0.020)	(0.037)	(0.035)
Current Cigarette Tax	0.015	0.015	0.016
	(0.013)	(0.011)	(0.010)
Individuals	8,228	8,228	8,228
Observations	89,289	89,289	89,289

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects in columns (1)-(3) are calculated using the delta method. Exponentiated coefficients for column (1) are hazard ratios and odds ratios for column (2).

Table B.3: Discrete-Time Hazard Model of Smoking Initiation for Additional Subsamples

	(1) Full Sample	(2) Ever Moved States	(3) Never Moved States	(4) Older Sibling	(5) No Older Sibling	(6) Born Before 1985	(7) Born in 1985 or After
A. Hazard Ratios ($H_0 : e^\beta = 1$)							
Cigarette Tax in Childhood	0.498**	0.444**	0.485**	0.583**	0.387**	0.496**	0.659**
	(0.080)	(0.122)	(0.123)	(0.123)	(0.108)	(0.221)	(0.169)
Current Cigarette Tax ($)	1.062	1.065	1.054	1.035	1.098	1.326**	1.083
	(0.053)	(0.080)	(0.088)	(0.062)	(0.093)	(0.158)	(0.059)
B. Marginal Effects ($H_0 : (e^\beta - 1) \times 0.25 = 0$)							
Cigarette Tax in Childhood	-0.125**	-0.139**	-0.129**	-0.104**	-0.153**	-0.126**	-0.085**
	(0.020)	(0.030)	(0.031)	(0.031)	(0.027)	(0.055)	(0.042)
Current Cigarette Tax ($)	0.015	0.016	0.014	0.009	0.024	0.082**	0.021
	(0.013)	(0.020)	(0.022)	(0.016)	(0.023)	(0.040)	(0.015)
Mean Smoking Initiation Hazard	0.054	0.055	0.054	0.053	0.056	0.077	0.044
Individuals	8,228	3,599	4,560	4,935	3,289	3,239	4,983
Observations	89,289	39,136	50,084	52,545	36,701	33,189	56,050

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. Coefficients are estimated with a complementary log-log regression. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Table B.4: Discrete-time Hazard Model of Smoking Initiation with Alternative Specifications

	(1) State-Time Trends	(2) Birth State	(3) State at Age 8	(4) Tax at Birth
	A. Hazard Ratios ($H_0 : e^{\beta} = 1$)			
Cigarette Tax in Childhood (Birth to Age 7)	0.391**	0.487**	0.447**	0.626**
	(0.078)	(0.082)	(0.085)	(0.112)
Current Cigarette Tax	1.247**	1.058**	1.110**	1.066**
	(0.089)	(0.052)	(0.052)	(0.047)
	B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)			
Cigarette Tax in Childhood (Birth to Age 7)	-0.152**	-0.128**	-0.138**	-0.094**
	(0.020)	(0.020)	(0.021)	(0.028)
Current Cigarette Tax	0.062**	0.015	0.027**	0.016
	(0.022)	(0.013)	(0.013)	(0.012)
Mean Smoking Initiation Hazard	0.054	0.054	0.054	0.054
Individuals	8228	8,228	8,228	7,605
Observations	89289	85,313	85,313	82,815

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include controls for sex, race, parent education, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method. Each model includes age fixed effects, column (1) includes state-specific linear time trends while columns (2)-(4) include state and year fixed effects. Columns (2) and (3) assume immobility in childhood where column (2) assigns the state of birth to ages 0 to 7 while column (3) assigns the state at age 8 to ages 0 to 7. Column (4) uses the tax in the year and state of birth as the tax during childhood instead of an average across the childhood years.

Table B.5: Summary Statistics Without Sample Weights

	(1) Full Sample	(2) Mother Ever Smoked	(3) Mother Never Smoked	(4) Born Before 1985	(5) Born 1985 or After
A. Individual Level					
Initiated in Sample	0.59	0.66	0.50	0.74	0.49
Left Sample Without Initiating	0.41	0.34	0.50	0.26	0.51
Average Cigarette Tax ($): Birth to Age 7	0.44	0.44	0.44	0.35	0.50
Hispanic	0.22	0.19	0.26	0.24	0.21
Black	0.32	0.31	0.33	0.39	0.28
Other Race (Including White)	0.46	0.50	0.40	0.37	0.51
Male	0.51	0.51	0.50	0.50	0.51
Mother's Age at Birth	25.76	25.38	26.39	20.41	29.23
Birth Order	2.03	2.06	2.01	1.61	2.31
Mother Ever Smoked	0.56	1.00	0.00	0.61	0.53
Parent Education: Less Than High School	0.06	0.07	0.05	0.08	0.05
Parent Education: High School	0.29	0.33	0.24	0.37	0.24
Parent Education: Some College	0.48	0.47	0.48	0.45	0.49
Parent Education: BA or More	0.17	0.13	0.22	0.10	0.22
Family Income: 1st Quartile	0.30	0.35	0.22	0.39	0.23
Family Income: 2nd Quartile	0.26	0.26	0.25	0.28	0.24
Family Income: 3rd Quartile	0.26	0.24	0.30	0.22	0.29
Family Income: 4th Quartile	0.18	0.14	0.24	0.10	0.24
Individuals	8,228	4,642	3,537	3,239	4,983
B. Individual-Age Level					
Current Cigarette Tax ($)	0.56	0.55	0.57	0.39	0.67
Smoking Initiation Hazard	0.010	0.012	0.006	0.013	0.007
Observations	89,289	47,330	41,910	33,189	56,050

Notes: Means of each variable are reported. Data from the NLSCYA. Years of analysis range from 1984 to 2014. Cigarette taxes are in real 2014 dollars.

Table B.6: Discrete-time Hazard Model of Smoking Initiation, Unweighted Data

	(1) Full Sample	(2) Mother Ever Smoked	(3) Mother Never Smoked
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)			
Cigarette Tax in Childhood (Birth to Age 7)	0.630**	0.566**	0.734
	(0.113)	(0.109)	(0.169)
Current Cigarette Tax	1.007	0.998	1.037
	(0.053)	(0.054)	(0.088)
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)			
Cigarette Tax in Childhood (Birth to Age 7)	-0.093**	-0.108**	-0.067
	(0.028)	(0.027)	(0.042)
Current Cigarette Tax	0.002	-0.001	0.009
	(0.013)	(0.014)	(0.022)
Mean Smoking Initiation Hazard	0.010	0.012	0.006
Individuals	8,228	4,642	3,537
Observations	89,289	47,330	41,910

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Table B.7: Discrete-time Hazard Model of Smoking Initiation, Unweighted Data

	(1) Full Sample	(2) Ever Moved States	(3) Never Moved States	(4) Older Sibling	(5) No Older Sibling	(6) Born Before 1985	(7) Born in 1985 or After
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)							
Cigarette Tax in Childhood (Birth to Age 7)	0.630**	0.536**	0.743	0.760	0.504**	0.546**	0.808
	(0.113)	(0.132)	(0.198)	(0.171)	(0.122)	(0.221)	(0.192)
Current Cigarette Tax	1.007	1.015	1.002	0.965	1.085	1.202	1.027
	(0.053)	(0.066)	(0.073)	(0.049)	(0.098)	(0.160)	(0.070)
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)							
Cigarette Tax in Childhood (Birth to Age 7)	-0.093**	-0.116**	-0.064	-0.060	-0.124**	-0.114**	-0.048
	(0.028)	(0.033)	(0.049)	(0.043)	(0.031)	(0.055)	(0.048)
Current Cigarette Tax	0.002	0.004	0.000	-0.009	0.021	0.051	0.007
	(0.013)	(0.016)	(0.018)	(0.012)	(0.024)	(0.040)	(0.018)
Mean Smoking Initiation Hazard	0.010	0.010	0.009	0.010	0.009	0.013	0.007
Individuals	8,228	3,599	4,560	4,935	3,289	3,239	4,983
Observations	89,289	39,136	50,084	52,545	36,701	33,189	56,050

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

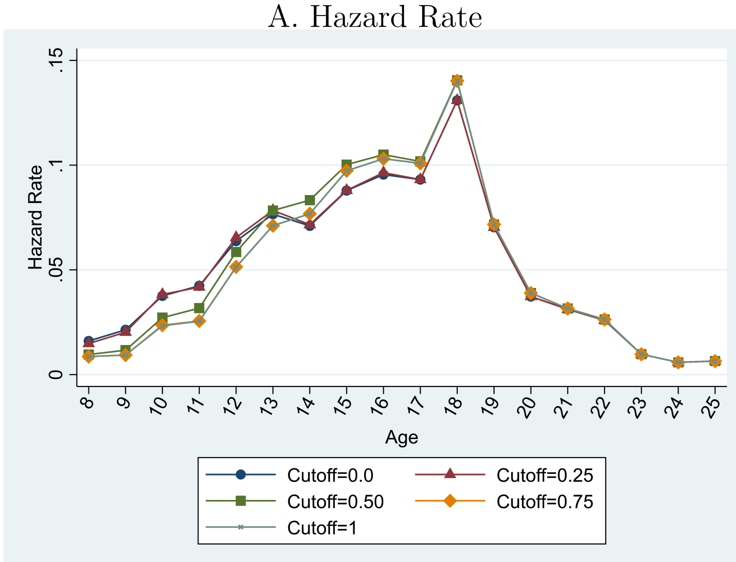
Table B.8: Summary Statistics, In and Out of the Estimation Sample

	(1) In Sample	(2) Out of Sample
A. Individual Level		
Initiated in Sample	0.58	0.65
Left Sample Without Initiating	0.42	0.35
Average Cigarette Tax ($): Birth to Age 7	0.45	0.45
Hispanic	0.08	0.07
Black	0.17	0.13
Other Race (Including White)	0.75	0.80
Male	0.51	0.53
Mother's Age at Birth	26.48	25.35
Birth Order	1.95	1.81
Mother Ever Smoked	0.59	0.58
Parent Education: Less Than High School	0.03	0.09
Parent Education: High School	0.27	0.32
Parent Education: Some College	0.49	0.43
Parent Education: BA or More	0.21	0.17
Family Income: 1st Quartile	0.18	0.25
Family Income: 2nd Quartile	0.23	0.27
Family Income: 3rd Quartile	0.32	0.25
Family Income: 4th Quartile	0.27	0.23
Individuals	8,228	3,278
B. Individual-Age Level		
Current Cigarette Tax ($)	0.87	1.01
Smoking Initiation Hazard	0.054	0.016
Observations	89,289	17,387

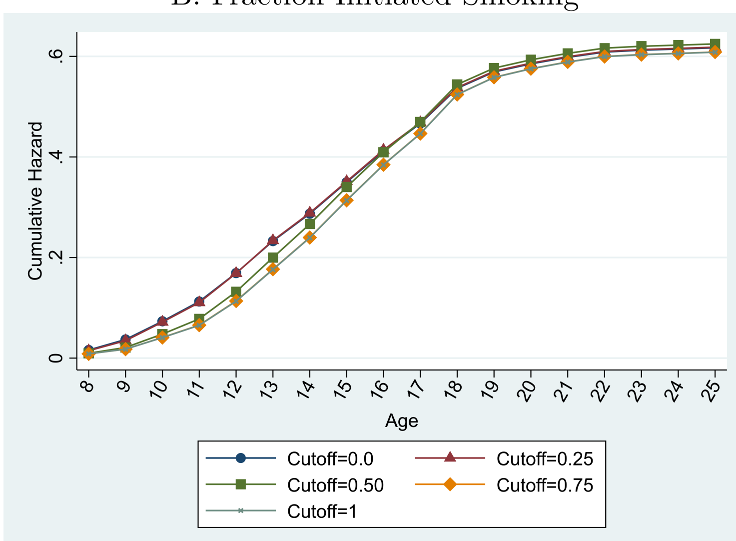
Notes: Means of each variable are reported. Data from the NLSCYA and weighted using NLSY79 weights for the mothers of those in our sample. Years of analysis range from 1984 to 2014. Cigarette taxes are in real 2014 dollars.

Figure B.1: Hazard Comparison for Several Failure Cutoffs

A. Hazard Rate



B. Fraction Initiated Smoking



Notes: Failure is defined as whether the fraction of reports of age first started smoking are above a certain cutoff. A cutoff of 0 corresponds to using the minimum age reported to define initiation and a cutoff of 1 corresponds to using the maximum age reported.

Table B.9: Discrete-time Hazard Model of Smoking Initiation with Alternative Failure Cutoffs

	(1) Cutoff at 0	(2) Cutoff at 0.25	(3) Cutoff at 0.5	(4) Cutoff at 0.75	(5) Cutoff at 1	(6) No Discrepancy
	A. Hazard Ratio ($H_0 : e^{\beta} = 1$)					
Cigarette Tax in Childhood	0.462**	0.465**	0.498**	0.525**	0.530**	0.501**
	(0.068)	(0.068)	(0.080)	(0.087)	(0.089)	(0.098)
Current Cigarette Tax	1.056	1.056	1.062	1.064	1.061	1.087
	(0.052)	(0.051)	(0.053)	(0.051)	(0.051)	(0.061)
	B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)					
Cigarette Tax in Childhood	-0.134**	-0.134**	-0.125**	-0.119**	-0.117**	-0.125**
	(0.017)	(0.017)	(0.020)	(0.022)	(0.022)	(0.024)
Current Cigarette Tax	0.014	0.014	0.015	0.016	0.015	0.022
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.015)
Mean Smoking Initiation Hazard	0.055	0.055	0.054	0.051	0.051	0.047
Individuals	8,123	8,143	8,228	8,237	8,237	7,256
Observations	86,707	86,910	89,289	89,939	89,973	83,048

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Table B.10: Discrete-time Hazard Model of Smoking Initiation, Only Those Observed Until Age 25

	(1) Full Sample
A. Hazard Ratios ($H_0 : e^{\beta} = 1$)	
Cigarette Tax in Childhood (Birth to Age 7)	0.511**
	(0.141)
Current Cigarette Tax	1.076
	(0.101)
B. Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)	
Cigarette Tax in Childhood (Birth to Age 7)	-0.122**
	(0.035)
Current Cigarette Tax	0.019
	(0.025)
Mean Smoking Initiation Hazard	0.066
Individuals	5,545
Observations	60,441

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.05$, $^{*}p < 0.1$. All models include state, age, and year fixed effects as well as controls for sex, race, parent education, mother's age at birth, birth order, mother smoking history, and family income. Cigarette taxes are in real 2014 dollars. The age of initiation is the age at least half of retrospective reports indicate smoking by that age. Standard errors for the marginal effects are calculated using the delta method.

Table B.11: Discrete-time Hazard Model, All Coefficients

	(1) Full Sample	(2) Mother Ever Smoked	(3) Mother Never Smoked
Marginal Effects ($H_0 : (e^{\beta} - 1) \times 0.25 = 0$)			
Cigarette Tax in Childhood	-0.125**	-0.138**	-0.109**
	(0.020)	(0.022)	(0.031)
Current Cigarette Tax ($)	0.015	0.006	0.035
	(0.013)	(0.015)	(0.026)
Mother's Age at Birth	-0.007**	-0.010**	-0.001
	(0.002)	(0.003)	(0.005)
Birth Order	0.025**	0.026**	0.020**
	(0.007)	(0.008)	(0.010)
Mother Ever Smoked	0.114**	0.000	0.000
	(0.016)	(0.000)	(0.000)
Male	0.050**	0.026**	0.104**
	(0.011)	(0.012)	(0.018)
Black	-0.111**	-0.121**	-0.093**
	(0.008)	(0.009)	(0.017)
Hispanic	-0.042**	-0.058**	-0.012
	(0.010)	(0.011)	(0.019)
Income, 2nd Quartile	-0.006	0.002	-0.028
	(0.015)	(0.025)	(0.023)
Income, 3rd Quartile	-0.053**	-0.049**	-0.068**
	(0.016)	(0.020)	(0.022)
Income, 4th Quartile	-0.068**	-0.075**	-0.066**
	(0.015)	(0.020)	(0.022)
Parent Education: High School	-0.013	-0.025	0.040
	(0.020)	(0.027)	(0.048)
Parent Education: Some College	-0.014	-0.023	0.044
	(0.019)	(0.026)	(0.044)
Parent Education: BA or More	-0.048**	-0.052*	-0.013
	(0.018)	(0.027)	(0.036)
Mean Smoking Initiation Hazard	0.054	0.065	0.041
Individuals	8,228	4,642	3,537
Observations	89,289	47,330	41,910

Table B.12: Summary Statistics for Additional Subsamples

	(1) Ever Moved States	(2) Never Moved States	(3) Older Sibling	(4) No Older Sibling	(5) Born Before 1985	(6) Born in 1985 or After
A. Individual Level						
Initiated in Sample	0.59	0.58	0.56	0.62	0.76	0.49
Left Sample Without Initiating	0.41	0.42	0.44	0.38	0.24	0.51
Average Cigarette Tax ($): Birth to Age 7	0.45	0.46	0.48	0.42	0.35	0.51
Hispanic	0.06	0.09	0.09	0.07	0.10	0.07
Black	0.15	0.18	0.18	0.15	0.23	0.14
Other Race (Including White)	0.78	0.73	0.73	0.78	0.67	0.79
Male	0.52	0.50	0.52	0.50	0.50	0.51
Mother's Age at Birth	26.50	26.59	28.06	24.28	20.85	29.50
Birth Order	1.94	1.96	2.63	1.00	1.55	2.16
Mother Ever Smoked	0.59	0.58	0.59	0.59	0.65	0.55
Parent Education: Less Than High School	0.02	0.05	0.04	0.02	0.05	0.02
Parent Education: High School	0.21	0.31	0.27	0.26	0.36	0.22
Parent Education: Some College	0.50	0.48	0.48	0.49	0.47	0.50
Parent Education: BA or More	0.27	0.16	0.21	0.23	0.12	0.26
Family Income: 1st Quartile	0.16	0.19	0.20	0.16	0.26	0.14
Family Income: 2nd Quartile	0.26	0.20	0.23	0.23	0.29	0.20
Family Income: 3rd Quartile	0.32	0.32	0.31	0.33	0.28	0.34
Family Income: 4th Quartile	0.26	0.28	0.26	0.28	0.17	0.33
Individuals	3,599	4,560	4,935	3,289	3,239	4,983
B. Individual-Age Level						
Current Cigarette Tax ($)	0.83	0.91	0.95	0.78	0.51	1.05
Smoking Initiation Hazard	0.055	0.054	0.053	0.056	0.077	0.044
Observations	39,136	50,084	52,545	36,701	33,189	56,050

Notes: Means of each variable are reported. Data from the NLSCYA and weighted using NLSY79 weights for the mothers of those in our sample. Years of analysis range from 1984 to 2014. Cigarette taxes are in real 2014 dollars.

Table B.13: First Stage Effect of Cigarette Taxes on Adult and Older Sibling Smoking

	(1) Male	(2) Female	(3) Male+Female	(4) Elasticity
	A. Adults (NLSY79)			
P(Current Smoker)	0.006	-0.011^{+}	-0.003	
	(0.009)	(0.006)	(0.005)	
Dep. Var. Mean	0.320	0.290	0.304	
N	26,793	28,497	55,290	
Number of Cigarettes	-0.274	-0.647**	-0.464**	-0.042*
	(0.320)	(0.192)	(0.192)	(0.018)
Dep. Var. Mean	13.285	11.679	12.492	2.175
N	7,205	7,040	14,245	14,245
	B. Siblings (NLSCYA)			
P(Current Smoker)	-0.012	-0.024**	-0.017*	
	(0.011)	(0.008)	(0.008)	
Dep. Var. Mean	0.28	0.21	0.24	
N	15,762	16,818	32,580	
Number of Cigarettes	0.01	-0.13	-0.02	-0.027
	(0.355)	(0.343)	(0.211)	(0.032)
Dep. Var. Mean	8.94	7.99	8.52	1.74
N	4,251	3,358	7,609	7,609

Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.01$, $^{*}p < 0.05$, $+p < 0.1$. All models include state and year fixed effects as well as controls for sex, race, and family income. Cigarette taxes are in real 2014 dollars.

Table B.14: First Stage Effect of Cigarette Taxes on Adult and Older Sibling Smoking – State-Time Trends

	(1) Male	(2) Female	(3) Male+Female	(4) Elasticity
	A. Adults (NLSY79)			
P(Current Smoker)	0.012	-0.002	0.004	
	(0.008)	(0.007)	(0.005)	
Dep. Var. Mean	0.320	0.290	0.304	
N	26,793	28,497	55,290	
Number of Cigarettes	-4.603**	-4.188**	-4.407**	-0.575**
	(0.920)	(0.830)	(0.866)	(0.111)
Dep. Var. Mean	13.29	11.68	12.49	2.18
N	7,205	7,040	14,245	14,245
	B. Siblings (NLSCYA)			
P(Current Smoker)	-0.018	-0.039**	-0.028**	
	(0.012)	(0.011)	(0.010)	
Dep. Var. Mean	0.281	0.207	0.243	
N	15,762	16,818	32,580	
Number of Cigarettes	-0.15	-0.40	-0.23	-0.060+
	(0.343)	(0.384)	(0.217)	(0.036)
Dep. Var. Mean	8.94	7.99	8.52	1.74
N	4,251	3,358	7,609	7,609

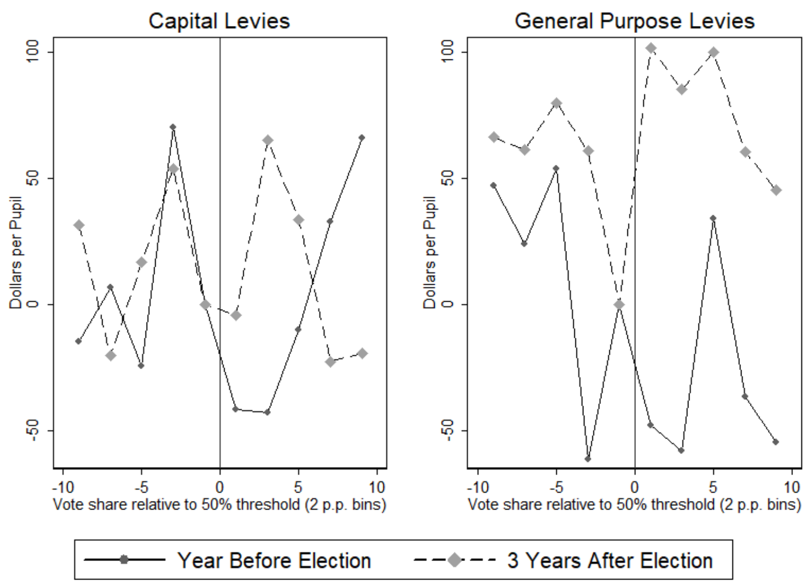
Notes: Robust standard errors clustered at the state level in parenthesis: $^{**}p < 0.01$, $^{*}p < 0.05$, $+p < 0.1$. All models include state-specific linear time trends as well as controls for sex, race, and family income. Cigarette taxes are in real 2014 dollars.

APPENDIX C

TRADITIONAL PUBLIC SCHOOL DISTRICT RESOURCES AND CHARTER SCHOOL COMPETITION – APPENDIX

Figure C.1: Spending, by Vote Share, Before and After Election

Support Service Spending Per Pupil



Notes: Graphs show spending per pupil for capital levies (left panels) and general purpose levies (right panels), by vote share in the focal election. Elections are grouped into bins of 2 percentage points. Averages control for year effects and are normalized to the first bin to the left of the 50% threshold.

Table C.1: Average Effect of Passing Levies on Value Added

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Capital Levies					General Levies		
	4th	5th	6th	7th	8th	4th	5th	6th	7th	8th
					A. ITT Estimates					
Levy Passed	-1.674	2.614	-0.363	0.617	-2.260	-1.959	0.201	-1.483	2.162	-0.905
	(2.106)	(2.098)	(2.072)	(1.786)	(1.923)	(1.518)	(1.690)	(1.520)	(1.460)	(1.263)
					B. Recursive TOT Estimates					
Levy Passed	0.631	1.633	-0.252	-0.692	-2.288*	-2.209*	-0.630	-2.075*	1.055	0.131
	(1.556)	(1.328)	(1.297)	(1.100)	(1.100)	(1.097)	(1.141)	(1.047)	(0.881)	(0.820)
Dep. Var. Mean	2.85	2.67	2.82	2.38	2.24	2.67	2.49	2.75	2.22	2.09
N	9,207	7,782	7,786	7,795	7,795	15,652	13,748	13,765	13,772	13,772

Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Table C.2: Average Effect of Passing Levies on Value Added in Math

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Capital Levies					General Levies		
	4th	5th	6th	7th	8th	4th	5th	6th	7th	8th
					A. ITT Estimates					
Levy Passed	-1.180	2.488	3.239	8.808**	-3.985	-1.782	1.377	-2.024	3.433	0.553
	(2.903)	(2.962)	(2.951)	(3.374)	(2.797)	(2.104)	(2.405)	(2.088)	(2.434)	(1.919)
					B. Recursive TOT Estimates					
Levy Passed	1.916	2.624	0.233	0.228	-1.096	-3.199*	0.137	-2.785*	0.608	0.433
	(2.206)	(1.918)	(1.701)	(1.952)	(1.654)	(1.381)	(1.436)	(1.366)	(1.632)	(1.279)
Dep. Var. Mean	4.15	3.54	3.31	3.73	3.36	3.87	3.27	3.17	3.52	3.11
N	9,207	7,782	7,786	7,795	7,795	15,652	13,748	13,765	13,772	13,772

Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Table C.3: Average Effect of Passing Levies on Value Added in Reading

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Capital Levies					General Levies				
	4th	5th	6th	7th	8th	4th	5th	6th	7th	8th
					A. ITT Estimates					
Levy Passed	-2.001	1.808	-2.311	-0.802	-0.323	0.791	-0.729	-1.302	0.364	-1.268
	(2.030)	(2.349)	(2.171)	(2.197)	(1.990)	(1.252)	(1.408)	(1.419)	(1.482)	(1.519)
					B. Recursive TOT Estimates					
Levy Passed	-1.155	1.650	-0.972	-0.016	-1.123	-0.013	-1.295	-2.062+	1.196	-1.433
	(1.194)	(1.416)	(1.301)	(1.259)	(1.159)	(0.837)	(1.075)	(1.082)	(1.021)	(1.041)
Dep. Var. Mean	2.56	3.11	3.46	2.97	2.43	2.46	2.89	3.38	2.78	2.33
N	9,214	7,787	7,786	7,795	7,795	15,657	13,750	13,765	13,772	13,772

Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

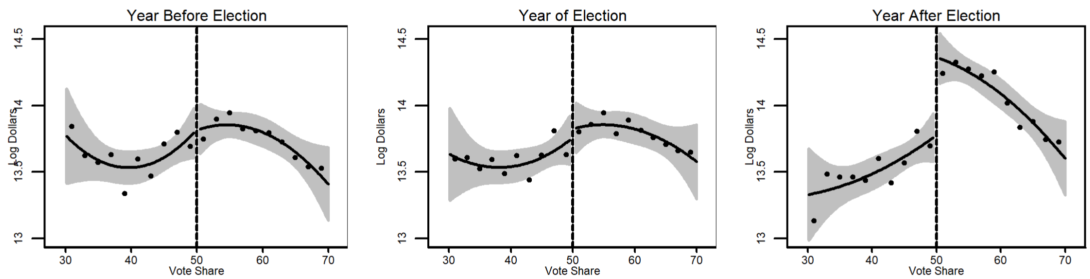
Table C.4: Average Effect of Passing Levies on Fraction of Students Lost to Charter Schools – First Election of Each Cycle

	(1)	(2)	(3)	(4)
	Capital Levies		General Levies	
	Digital	Brick and Mortar	Digital	Brick and Mortar
	A. ITT Estimates			
$1,000 PP Levy Passed	-0.011	0.005	-0.008	-0.015
	(0.008)	(0.014)	(0.006)	(0.013)
	B. Recursive TOT Estimates			
$1,000 PP Levy Passed	-0.015**	0.004	-0.011**	-0.010
	(0.004)	(0.008)	(0.004)	(0.006)
Dep. Var. Mean	0.010	0.006	0.012	0.010
N	12,288	12,288	16,424	16,424

Notes: Coefficients from OLS regressions are reported with standard errors in parenthesis: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Spending is in real thousands of dollars per pupil.

Figure C.2: Log of Capital Spending by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

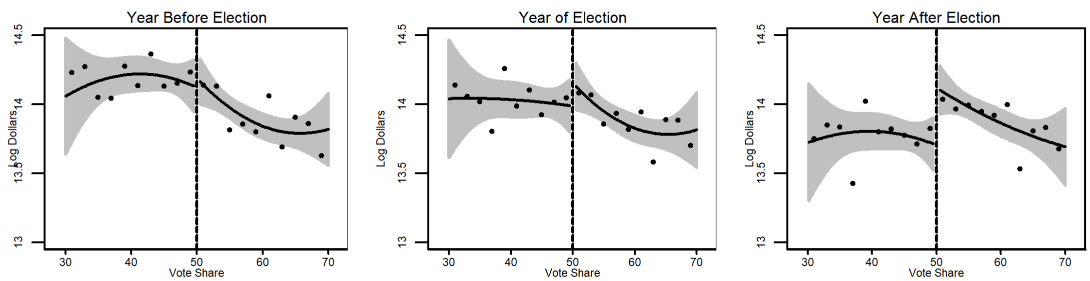
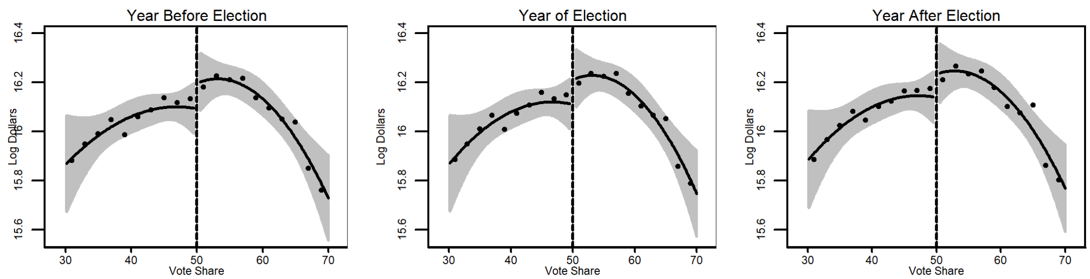


Figure C.3: Log of Instructional Expenditure by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

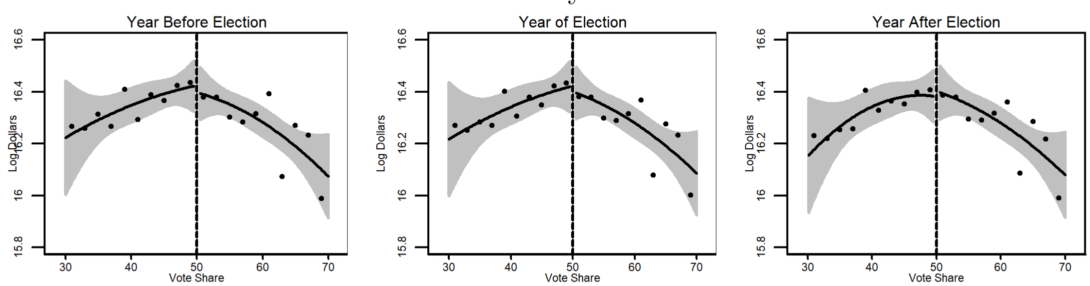
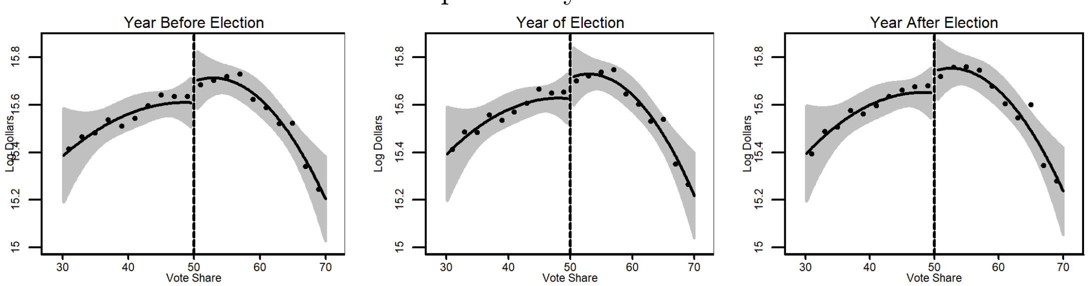


Figure C.4: Log of Support Service Expenditure by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

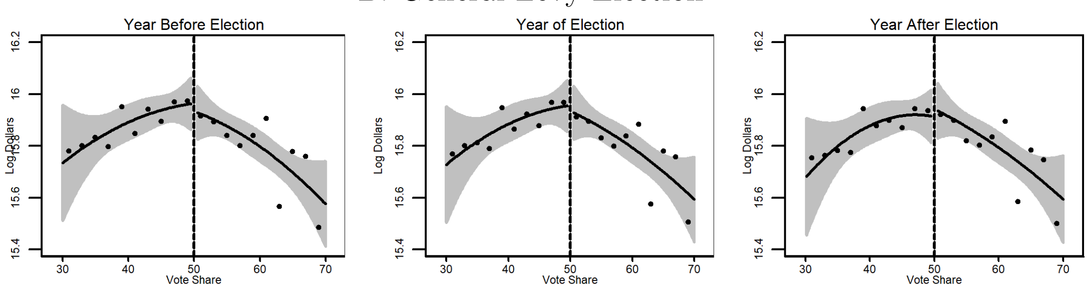
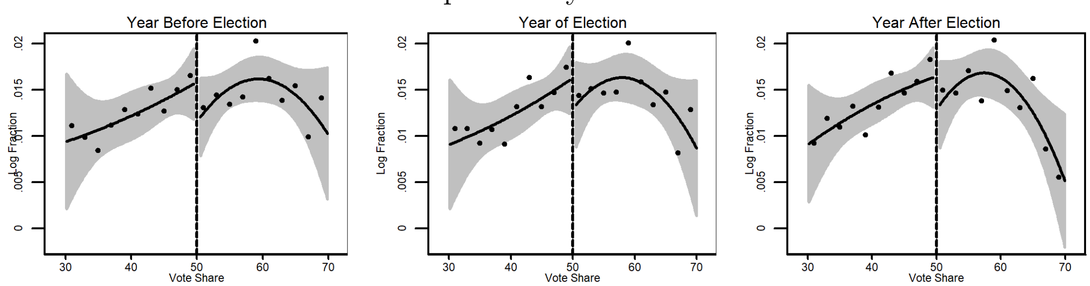


Figure C.5: Fraction of Students Lost to Any Charter by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

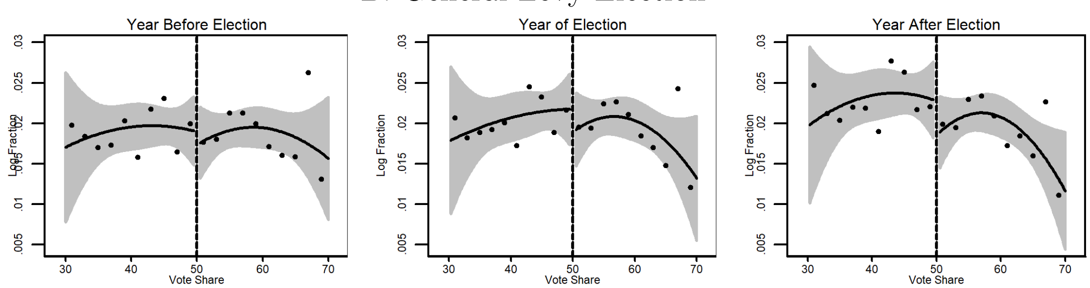


Figure C.6: Fraction of Students Lost to Charters by Vote Share Before and After Elections

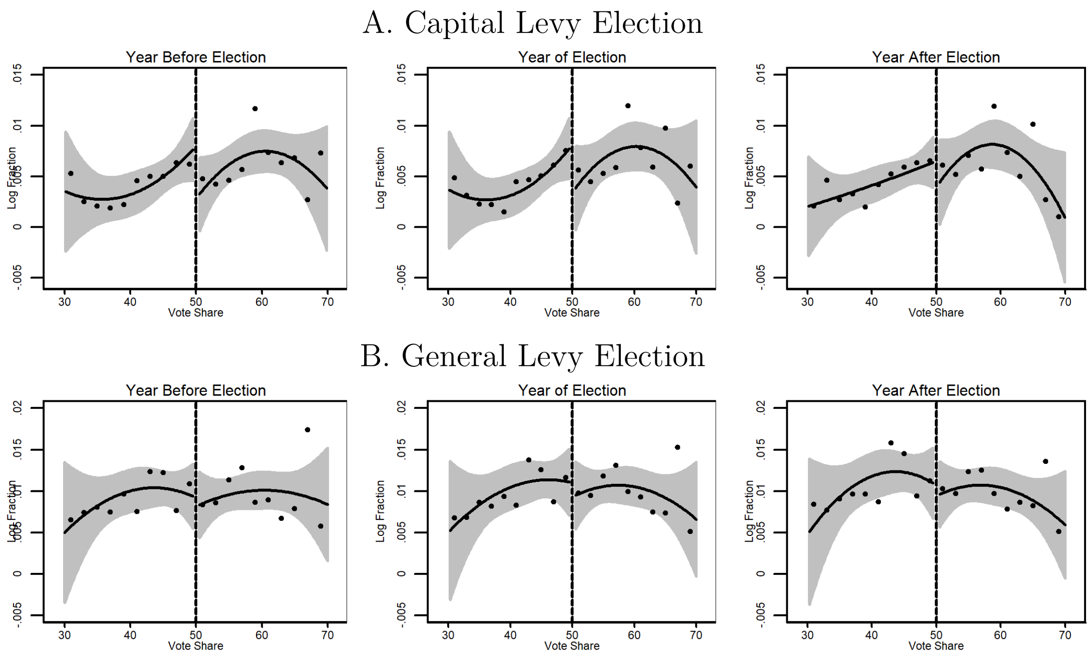
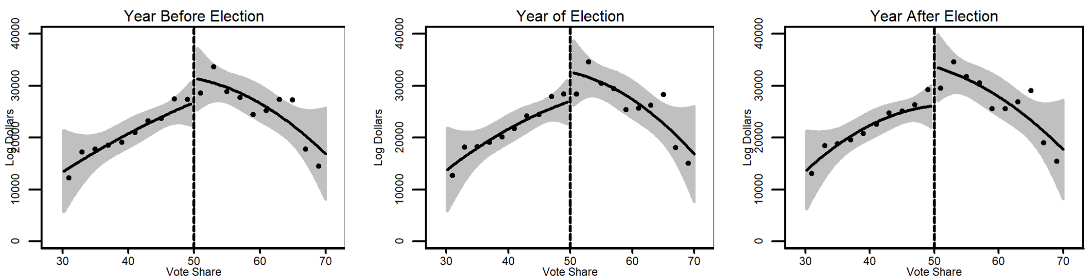


Figure C.7: Residential Property Values by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

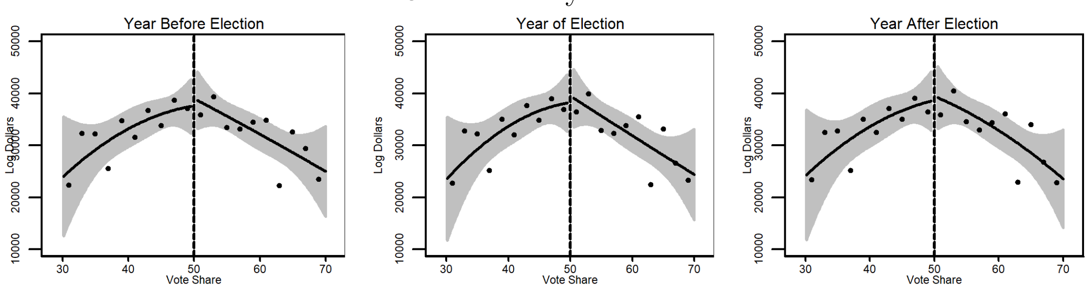
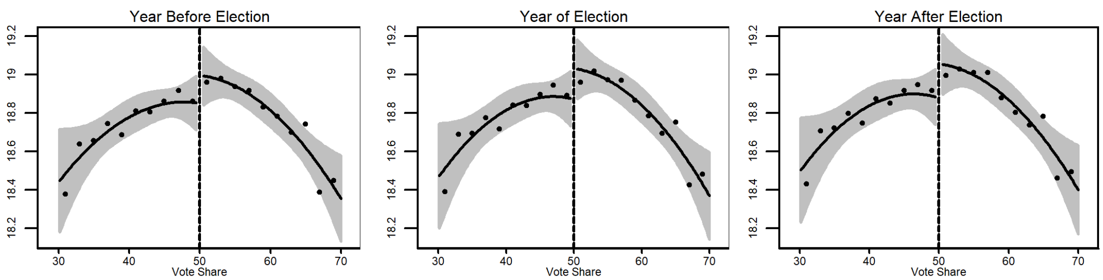


Figure C.8: Log Residential Property Values by Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

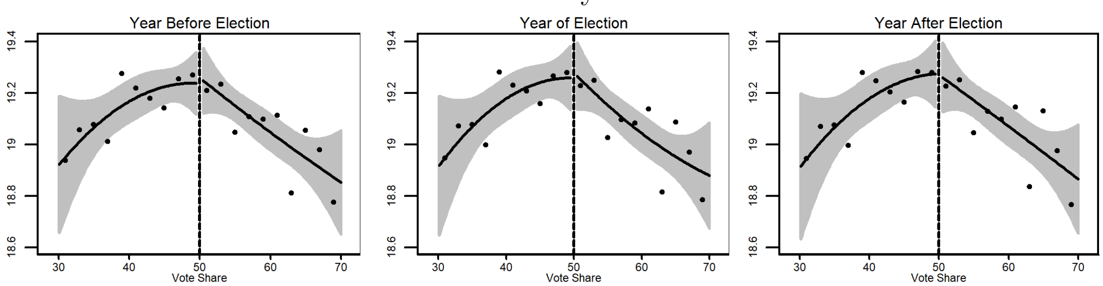
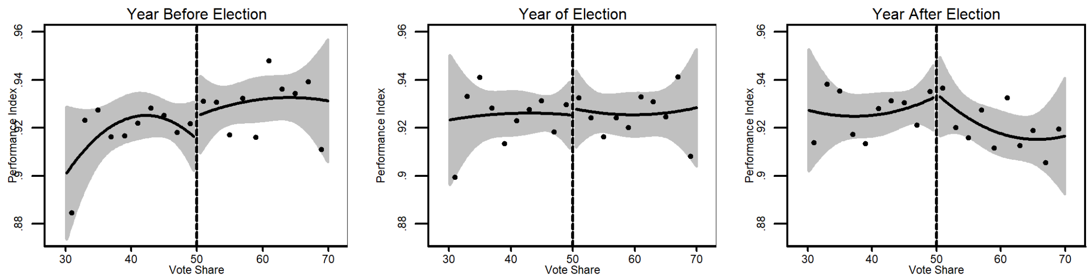


Figure C.9: District Performance Index Before and After Elections

a. Capital Levy Election



b. General Levy Election

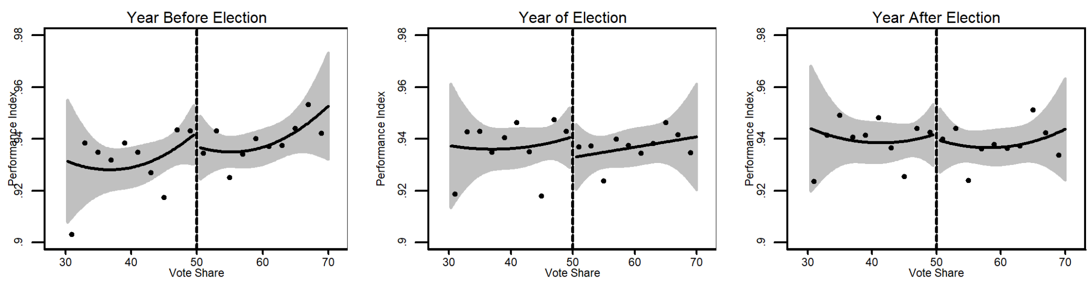
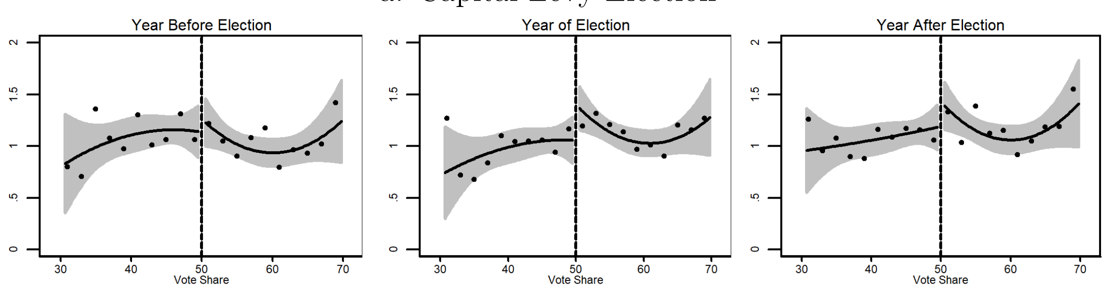


Figure C.10: District Value Added Before and After Elections

a. Capital Levy Election



b. General Levy Election

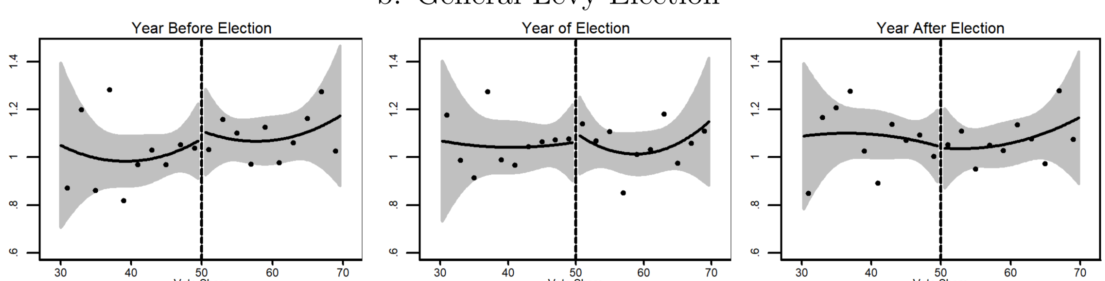
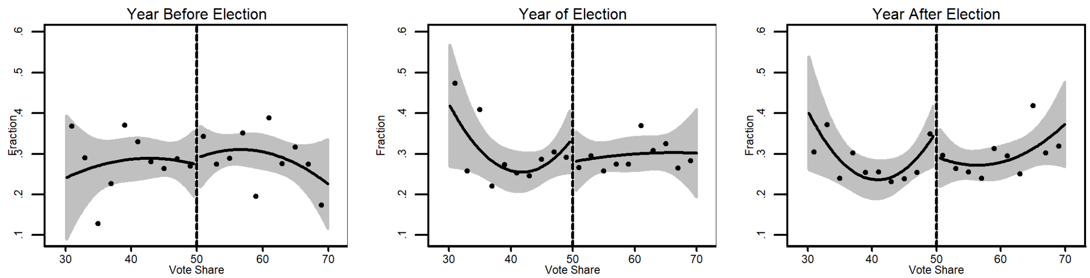


Figure C.11: Fraction Making AYP Across Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

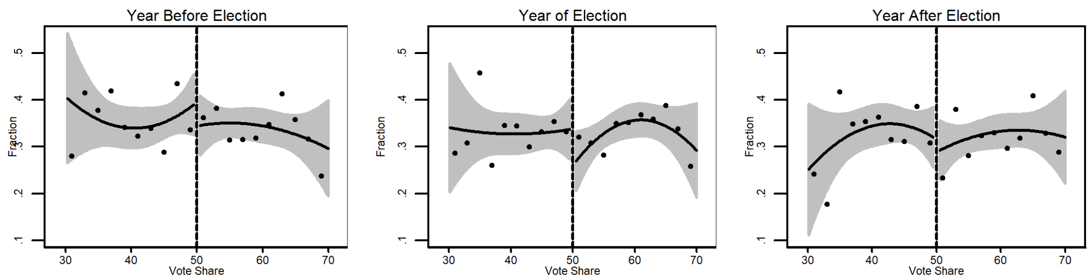
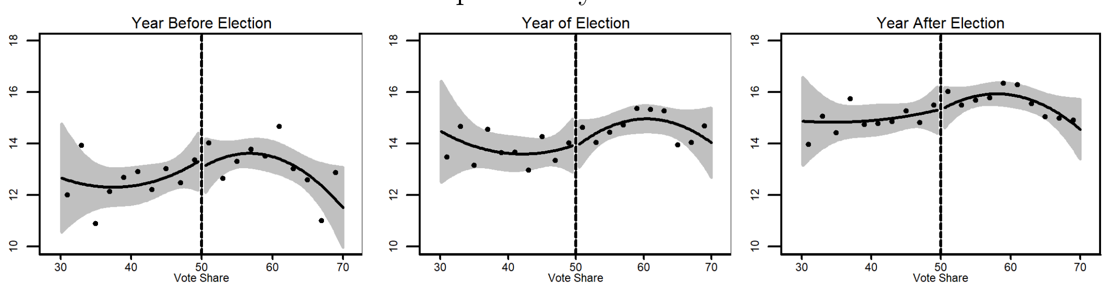


Figure C.12: Student-Teacher Ratio Before and After Elections

a. Capital Levy Election



b. General Levy Election

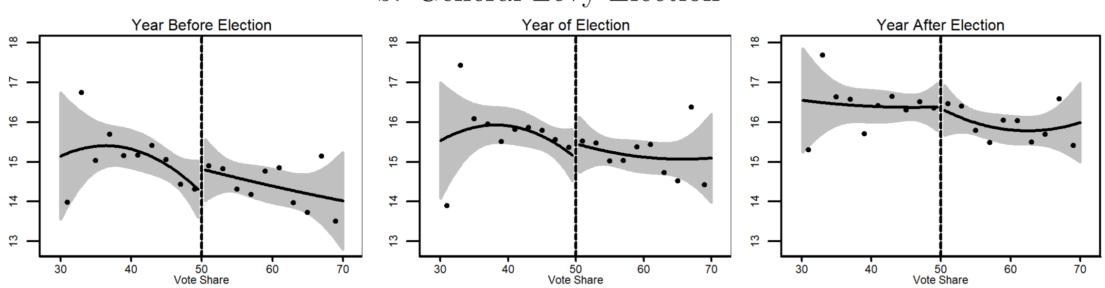
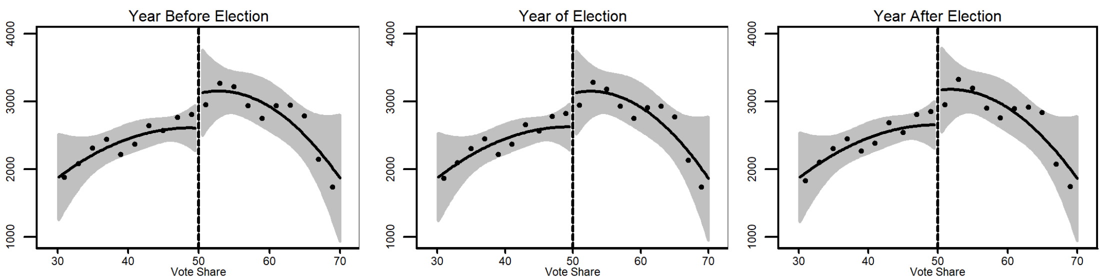


Figure C.13: Total Number of Students Before and After Elections

a. Capital Levy Election



b. General Levy Election

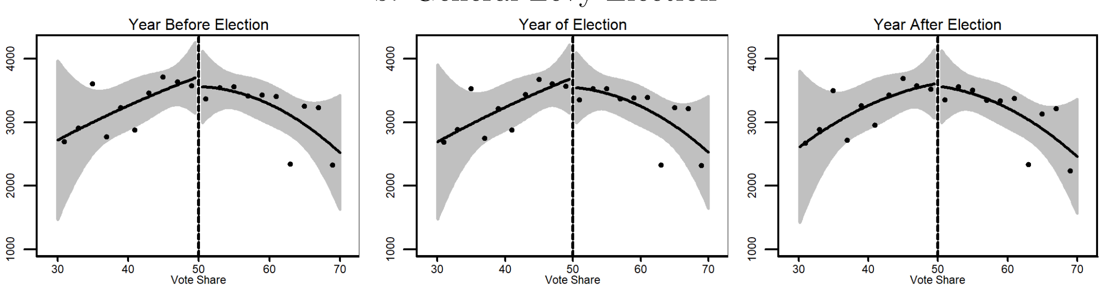
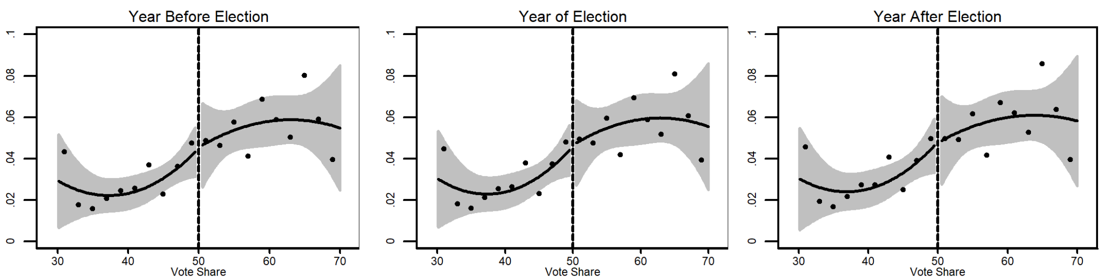


Figure C.14: Fraction of Black Students Across Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

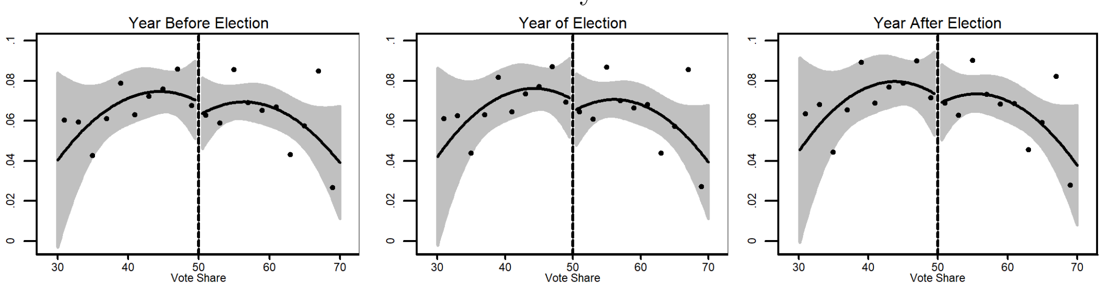
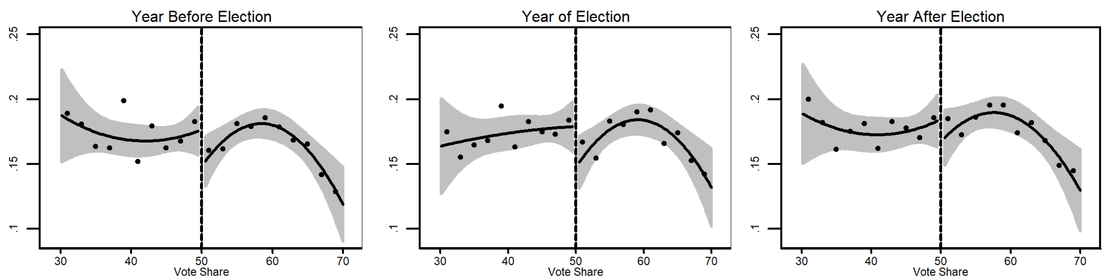


Figure C.15: Fraction of FRPL Eligible Students Across Vote Share Before and After Elections

A. Capital Levy Election



B. General Levy Election

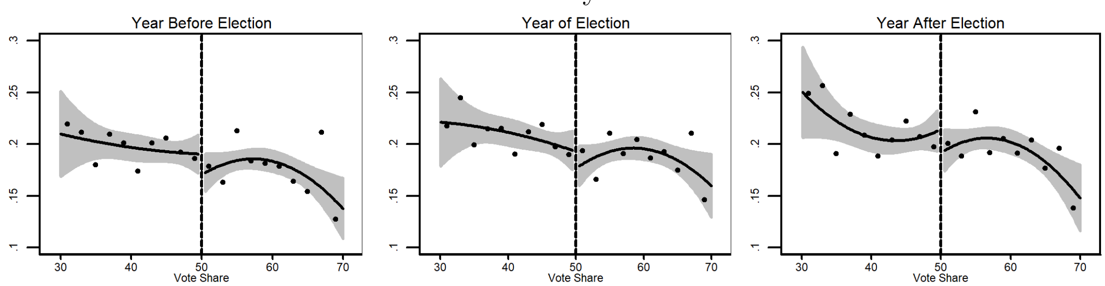


Table C.5: Dynamic Effect of Levy Passage on Per-pupil Spending

	(1)	(2)	(3)	(4)
	Capital Levies		General Levies	
	Capital	Instructional	Capital	Instructional
	A. ITT Estimates			
Levy Passed 1 Year Ago	42.24**	1.21	231.82	103.56*
	(16.32)	(7.45)	(176.78)	(50.74)
Levy Passed 2 Years Ago	262.28**	0.88	226.71	154.21**
	(73.18)	(6.21)	(166.56)	(52.75)
Levy Passed 3 Years Ago	410.21**	-0.77	79.85	119.07**
	(69.50)	(8.99)	(167.68)	(41.12)
Levy Passed 4 Years Ago	172.54**	10.91	31.05	84.18*
	(47.10)	(13.61)	(162.13)	(41.75)
Levy Passed 5 Years Ago	4.71	-0.31	18.17	81.57
	(29.59)	(6.18)	(183.20)	(53.35)
Levy Passed 6 Years Ago	-8.22	4.10	-170.52	139.89
	(21.48)	(14.06)	(266.75)	(110.57)
	B. Recursive TOT Estimates			
Levy Passed 1 Year Ago	0.58	1.63	365.04*	106.38*
	(10.91)	(13.87)	(161.37)	(43.54)
Levy Passed 2 Years Ago	52.41**	0.69	388.91**	140.91**
	(15.92)	(7.23)	(123.85)	(37.45)
Levy Passed 3 Years Ago	275.06**	1.53	484.89**	208.14**
	(70.70)	(5.96)	(113.12)	(32.35)
Levy Passed 4 Years Ago	450.69**	-0.83	481.04**	173.64**
	(66.98)	(8.77)	(113.05)	(28.59)
Levy Passed 5 Years Ago	203.90**	11.46	607.66**	152.79**
	(41.55)	(13.10)	(118.13)	(30.56)
Levy Passed 6 Years Ago	18.31	0.40	588.46**	143.50**
	(23.08)	(5.90)	(141.96)	(38.53)
N	20,714	20,714	28,123	28,123

Table C.6: Dynamic Effect of Levy Passage on Students Lost to Charter Schools

	(1)	(2)	(3)	(4)
	Capital Levies		General Levies	
	Digital	Brick and Mortar	Digital	Brick and Mortar
	A. ITT Estimates			
Levy Passed 1 Year Ago	0.000	0.000	-0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.001)
Levy Passed 2 Years Ago	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)
Levy Passed 3 Years Ago	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)
Levy Passed 4 Years Ago	0.000	0.000	-0.001	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)
Levy Passed 5 Years Ago	-0.000	0.000	-0.000	0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Levy Passed 6 Years Ago	0.000	0.000	0.000	0.002
	(0.000)	(0.000)	(0.001)	(0.002)
	B. Recursive TOT Estimates			
Levy Passed 1 Year Ago	0.000	0.000	0.000	0.001*
	(0.000)	(0.000)	(0.000)	(0.001)
Levy Passed 2 Years Ago	-0.000	0.000	-0.001**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Levy Passed 3 Years Ago	-0.000	0.000	-0.001**	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)
Levy Passed 4 Years Ago	-0.000	0.000	-0.001*	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)
Levy Passed 5 Years Ago	-0.000	0.000	-0.002**	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)
Levy Passed 6 Years Ago	-0.000+	0.000	-0.002*	0.000
	(0.000)	(0.000)	(0.001)	(0.001)
N	15,168	15,168	22,381	22,381

Table C.7: Dynamic Effect of Levy Passage on Student Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Capital Levies			General Levies		
	Performance Index	Value Added	AYP Met	Performance Index	Value Added	AYP Met
	A. ITT Estimates					
Levy Passed 1 Year Ago	0.000	-0.001	-0.002	0.002	0.029	0.027
	(0.000)	(0.011)	(0.002)	(0.002)	(0.081)	(0.031)
Levy Passed 2 Years Ago	0.000	-0.005	-0.001	0.004+	-0.088	0.030
	(0.000)	(0.011)	(0.002)	(0.002)	(0.096)	(0.032)
Levy Passed 3 Years Ago	-0.000	0.005	-0.004	0.003	-0.005	-0.005
	(0.000)	(0.012)	(0.002)	(0.003)	(0.104)	(0.038)
Levy Passed 4 Years Ago	0.000	0.009	-0.002	0.002	0.089	0.018
	(0.000)	(0.012)	(0.003)	(0.003)	(0.131)	(0.041)
Levy Passed 5 Years Ago	0.000	-0.001	-0.000	0.001	0.136	-0.012
	(0.000)	(0.014)	(0.003)	(0.003)	(0.132)	(0.044)
Levy Passed 6 Years Ago	0.000	-0.002	0.001	0.005	0.119	0.034
	(0.000)	(0.014)	(0.004)	(0.003)	(0.152)	(0.051)
	B. Recursive TOT Estimates					
Levy Passed 1 Year Ago	0.004	0.009	0.001	0.017	-0.124	0.029
	(0.003)	(0.017)	(0.004)	(0.019)	(0.087)	(0.029)
Levy Passed 2 Years Ago	-0.000	0.003	-0.002	0.028*	0.012	0.009
	(0.002)	(0.009)	(0.002)	(0.014)	(0.058)	(0.022)
Levy Passed 3 Years Ago	0.002	-0.006	-0.001	0.026	-0.088	0.021
	(0.004)	(0.008)	(0.002)	(0.016)	(0.071)	(0.022)
Levy Passed 4 Years Ago	-0.004	-0.008	-0.003	0.035+	-0.052	0.029
	(0.003)	(0.010)	(0.003)	(0.019)	(0.077)	(0.026)
Levy Passed 5 Years Ago	-0.000	-0.000	0.001	0.029	-0.050	0.051+
	(0.002)	(0.011)	(0.003)	(0.022)	(0.092)	(0.029)
Levy Passed 6 Years Ago	0.002	-0.010	0.002	0.020	-0.045	0.052+
	(0.003)	(0.012)	(0.003)	(0.022)	(0.100)	(0.030)
N	15,520	7,795	17,537	21,852	13,772	23,623

Table C.8: Dynamic Effect of Levy Passage on Student and Teacher Counts

	(1)	(2)	(3)	(4)	(5)	(6)
		Capital Levies			General Levies	
	S/T Ratio	# of Students	# of Teachers	S/T Ratio	# of Students	# of Teachers
			A. ITT Estimates			
Levy Passed 1 Year Ago	0.005	-4.205*	-2.071	-0.171+	0.170	5.348
	(0.008)	(1.930)	(2.526)	(0.094)	(22.262)	(25.920)
Levy Passed 2 Years Ago	-0.001	-3.494	-3.598	-0.159+	22.330	7.011
	(0.011)	(2.510)	(2.957)	(0.094)	(28.692)	(30.024)
Levy Passed 3 Years Ago	0.005	-3.919	-5.712+	-0.275*	39.775	-76.519
	(0.010)	(3.490)	(3.398)	(0.138)	(37.372)	(56.678)
Levy Passed 4 Years Ago	0.009	-4.352	-5.724	-0.222	29.953	-83.338
	(0.012)	(4.317)	(3.559)	(0.140)	(46.397)	(63.047)
Levy Passed 5 Years Ago	0.021	-3.320	-6.496+	-0.084	42.546	-88.779
	(0.016)	(5.430)	(3.754)	(0.129)	(55.145)	(66.718)
Levy Passed 6 Years Ago	0.008	-4.610	-6.294	-0.162	40.410	-94.050
	(0.014)	(6.722)	(3.972)	(0.168)	(64.404)	(72.118)
			B. Recursive TOT Estimates			
Levy Passed 1 Year Ago	-0.014	-9.748**	0.255	-0.184+	-56.800*	-18.629
	(0.012)	(3.758)	(2.910)	(0.098)	(24.421)	(23.132)
Levy Passed 2 Years Ago	0.005	-4.320*	-1.728	-0.243**	3.757	2.675
	(0.008)	(1.955)	(2.374)	(0.072)	(16.178)	(11.167)
Levy Passed 3 Years Ago	-0.001	-3.382	-2.167	-0.278**	36.095	18.972
	(0.010)	(2.572)	(2.450)	(0.072)	(24.671)	(16.142)
Levy Passed 4 Years Ago	0.002	-4.343	-3.728	-0.300**	57.770+	-30.149
	(0.010)	(3.629)	(2.584)	(0.095)	(30.467)	(35.401)
Levy Passed 5 Years Ago	0.007	-5.195	-3.786	-0.318**	57.233	-33.963
	(0.011)	(4.533)	(2.753)	(0.105)	(39.641)	(38.960)
Levy Passed 6 Years Ago	0.026+	-4.751	-4.434	-0.152	66.697	-37.100
	(0.016)	(5.691)	(2.925)	(0.107)	(44.369)	(41.547)
N	20,828	20,848	20,848	28,275	28,280	28,280

Table C.9: Dynamic Effect of Levy Passage on Residential Values

	(1)	(2)	(3)	(4)	(5)	(6)
	Capital Levies			General Levies		
	\$10,000	Log	Per Pupil	\$10,000	Log	Per Pupil
			A. ITT Estimates			
Levy Passed 1 Year Ago	32.820	-0.000	-265.773	768.099**	0.002	1018.761
	(20.735)	(0.000)	(396.204)	(260.239)	(0.003)	(1276.882)
Levy Passed 2 Years Ago	102.762*	-0.000	240.443	628.936*	0.002	1052.707
	(44.834)	(0.000)	(153.325)	(290.204)	(0.006)	(2156.253)
Levy Passed 3 Years Ago	120.868*	0.000	-189.214	643.947	0.001	-1.1e+03
	(56.034)	(0.001)	(332.641)	(429.415)	(0.006)	(1509.354)
Levy Passed 4 Years Ago	150.416*	0.000	501.337	1197.804*	-0.002	-1.5e+03
	(67.021)	(0.001)	(321.239)	(607.593)	(0.008)	(1558.064)
Levy Passed 5 Years Ago	209.774*	0.001	1514.502	736.428	-0.000	-1.3e+03
	(87.066)	(0.001)	(1403.965)	(688.854)	(0.009)	(1933.565)
Levy Passed 6 Years Ago	189.955*	0.001	1042.974	870.491	0.000	659.049
	(81.374)	(0.001)	(927.657)	(746.166)	(0.011)	(3001.572)
			B. Recursive TOT Estimates			
Levy Passed 1 Year Ago	-5.369	-0.001*	-429.888	382.880+	-0.003	2858.342**
	(37.874)	(0.001)	(631.075)	(220.570)	(0.003)	(983.035)
Levy Passed 2 Years Ago	21.582	-0.000+	-306.109	609.151*	0.001	788.623
	(19.365)	(0.000)	(393.408)	(236.224)	(0.002)	(892.106)
Levy Passed 3 Years Ago	78.152+	-0.000	160.652	597.297*	0.002	297.340
	(42.874)	(0.000)	(144.673)	(244.935)	(0.004)	(1420.386)
Levy Passed 4 Years Ago	90.096+	-0.000	-286.376	528.649+	0.002	-1.3e+03
	(52.029)	(0.001)	(340.948)	(312.799)	(0.004)	(1006.006)
Levy Passed 5 Years Ago	119.384+	-0.000	389.950	934.248*	0.003	-1.6e+03
	(64.047)	(0.001)	(308.073)	(429.360)	(0.005)	(1051.770)
Levy Passed 6 Years Ago	157.154+	0.000	1332.901	558.997	0.002	-1.9e+03
	(82.508)	(0.001)	(1355.016)	(487.147)	(0.006)	(1284.913)
N	19,268	19,268	19,268	25,163	25,163	25,163

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